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T R E A T I S E
UPON
ARTIFICIAL ELECTRICITY,
IN WHICH ARE GIVEN
SOLUTIONS of a Number of interesting ELECTRIC
PHOENOMENA, hitherto unexplained.
TO WHICH IS ADDED,
A N E S S A Y
ON THE
MILD and SLOW ELECTRICITY which prevails in the
Atmosphere during Serene Weather.

Translated from the ORIGINAL ITALIAN of
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K

L O N D O N,
Printed for J. NOURSE, Bookfeller to HIS MAJESTY.
MDCCLXXVI.



I think I cannot do better than to premise to this book, the sentiments contained in the letter with which I accompanied the same, when I sent a copy of it to Dr. Franklin.

*Turin, 20th.
of May, 1771.*

I THANK you, most excellent Sir, for the exact description of your new and really harmonious musical instrument with glasses, which you have sent me (to you it is given to inform mankind of the true principles of the Electric Science, to encourage them against the terrors of thunder, and charm their ears with a most sweet music); and I will moreover venture to thank you likewise, in the name of our Italy, for having in consideration, as you are pleased to say, of our harmonious language, given the appellation of *armonica*, to your agreeable instrument*. With regard to me, I do not know how to testify better my gratitude to you, for the many marks of your kindness, than by sending to you this new product of my labour, and accompanying it with a wish, that it may answer your first opinion of it, as you were pleased to express it.

As for the bigness of this volume, I hope it will not tire you; besides the common excuse, that I have not had time enough to write with more brevity, I have had rather to repeat, than to quote: and to say the truth, I do not think that the number of both experiments and solutions contained in this Book, bear a very unequal proportion to the size of the volume.

I hope you will not be surpris'd that I conclude this book with expressing wishes that farther new confirmations of the Theory contained in it, may be procured; you too

* See Dr. Franklin's Works, page 438, fifth Edition.

well teach, by your own example and authority, how slowly and difficulty our senses allow us to proceed in the attainment of natural knowledge, which is inexhaustible. And how many new informations does not Science, however comprehensive, want to this day? We know how to measure the velocity of light, but we are ignorant how it proceeds. We can define the order of our mundane system, and yet we are ignorant of that force which both parts and unites it, &c.

But what should be the consequence, if new informations with regard to Electricity happened to produce another different theory? The whole series of my experiments to this day, takes any such suspicion from me; in such case however, the value of this book, if indeed it may be said to have any, would subsist. The experiments would remain, together with their collection and unity.

I hear that you are preparing to go back to your native country. Whether America or Europe possesses you, preserve yourself to the world, to science, and the lovers of it. Wherever you may be, I always shall be the most obsequious admirer of your eminent merit.

CON-

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OF

ARTIFICIAL ELECTRICITY.

SECTION I.

Of the Theory of Artificial Electricity, especially in *different* or conducting bodies, deduced from the circulation of the electrical fire in the ordinary apparatus.

CHAP. I.

Compendium of the theory.

1. **F**IRST, the two masters of true learning, Observation and Experience, have shewn this century, that within the substance, and on the surface of all bodies, a particular fluid element is diffused, which is of a distinct nature, and in the atmosphere governs the clouds, lightning, thunder, &c.

2. This element is diffused in bodies in proportion to their natural capacity, it remains there balanced with itself, has no motion, and produces no impression on our organs.

3. But when the power of nature, or of art, changes the natural proportion in bodies, by transporting part of that fluid from the one to the other, then finding itself unbalanced, it vehemently endeavours to spread and diffuse itself from the bodies in which its proportion is become greater, to those where it is become less.

4. And then, either that fluid is prevented, by resisting bodies that surround it, from diffusing itself to an equality; or, actually surmounting those resistances, becomes able to diffuse itself; and in both these cases it manifests, either its state of inæquilibrium,

or its actual diffusion, with certain peculiar signs, which are commonly called *signs of electricity*.

5. That element, which is now acknowledged as the principle of the greatest effects in nature, many of which art, borrowing help from it, has enabled itself to imitate, is the very same of which the ancients had indeed some knowledge, but were acquainted with only a few of its effects and motions, in a few particular bodies, chiefly in amber, or *electrum*; whence it has received, in these days, the name of electrical fire or fluid; the whole of its effects is called *electricity*; and the knowledge of these may be considered under two heads, and called Artificial Electricity, when and where it is excited by art; and Natural Electricity, when it is excited by nature herself.

6. Therefore, if the surrounding resisting bodies hinder the electrical fire, when unbalanced, from spreading itself to an equality, then it manifests this its unequal state, by making the moveable parts of the electrified body, diverge from each other, and tend towards bodies that are not electrified. I call that sign, an electrical sign of simple pression, or of mere inequality, or of *pressing electricity*.

7. Hence arose the idea of annexing to the bodies that are electrified, two threads, in order to estimate their present electricity from the degree of their divergency. I use for that purpose Flanders thread, extremely fine and flexible, two or three inches long; and, to prevent their entangling together, I suspend to each of them a little ball made of the pith of elder. This I call an *electroscope*: such an electroscope does not exceed in weight the fourth part of a grain.

8. But when the electric fire is actually able to surmount the surrounding resistances, and from the body in which it finds itself in a greater proportion, actually diffuses itself into bodies wherein its proportion is less; then it manifests itself with new signs, with particular motions, sparks, a wind, &c. all which are called vivid signs, signs of a present diffusion, signs of a *vivid electricity*.

9. And with regard to its motion; if around the electrified body, or near it, there are some moveable bodies, which the unbalanced electric fire may direct, extend, suspend, dispose, or throw into its way

way from the electrified body to bodies not electrified, that fire, by its peculiar efficiency, actually directs, extends, suspends, or throws those moveable bodies; and, in short, disposes them throughout the resisting medium in the properest manner, in order to diffuse itself through them, with the greatest velocity its nature, and the *tradtive* quality of those bodies, can allow.

10. But if around, or near, the electrified body, there are no moveable bodies, apt to transmit the electric fire, and which this may dispose in its way; and if at the same time this unbalanced fire is able to throw aside the resisting medium that parts it from bodies not electrified; then that fire actually drives this medium in that part where it resists less, with a noise and sparkling proportioned to its own density and quantity.

11. This sign arising from *sparks*, is often complicated with that resulting from *motion*; that is, whenever the series of bodies which the electric fire disposes in its way, is not continued enough, or of a sufficient size, invisibly to transmit the spark through its own substance; then, in the places where such a series is either too scanty, or interrupted, the fire rushes out, and manifests itself with a proportioned noise and light.

12. And it is in such a passage, through bodies of a small size, that the electrical fire may instantly produce in such bodies all the identical effects which the common fire can, though in a tract of time, produce in them, respectively; that is, the electrical fire condensed in a spark, alters these bodies proportionably to the density with which it presses through them, and conformably to their nature, in the same manner as the common fire would do, but with such a superior quickness, as it may be said to perform it in the shortest *tempusculum* of its passage.

13. Lastly, the third electric sign of an actual *diffusion*, is a real wind that blows, especially from points annexed to electrified bodies, or presented to them. The reason is this: those points throw into the contiguous air, or draw from the same, the electrical fire with such a force, that in both cases they repel from themselves the air, either loaded with part of the fire of which themselves are too replete, or deprived of its own fire, if these bodies were themselves in a *wanting* state; and these points, while

they thus drive away the air next to them, throw into, or draw from, the new air which laterally succeeds, the electrical fire, and effect it in such a divided manner, that the fire, whether drawn in, or thrown out, produces but a little noise, and affords but weak lights; but all this is effected with such rapidity, that a great quantity of fire is really thrown out, or drawn, in a very short tract of time.

14. I say, weak lights, and such they are if we consider the much less vivacity of the sparks. Besides, the light which appears above a *wanting* point, that draws in the fire, is of a small bulk, so that I express it by the name of *little star*; but the light emitted by a *superabounding* point from which the fire sparkles out, is much larger, consequently I call it, the *brush*. And I have made use of such a difference between the electric lights, in order to divide the *superabounding* bodies which throw out the electric fire, and which I call electrified *per excessum*, from the *wanting* bodies which draw it in, and which I call electrified *per defectum*. But of this I shall treat more at large when I give the demonstration of the theory, of which this is only a compendium.

15. In order to continue this compendium, it is now necessary to explain what are the *resistances* which I have mentioned, that prevent the electrical fire from diffusing itself to an equality, and stop and keep it in a state of *inæquilibrium*.

16. Now, experience has shewn, that the electrical fire, though universally diffused in all bodies, has not the same mobility in all. In metallic bodies, the electrical fire moves itself with the greatest facility and freedom; so that if the smallest quantity of it is added to a part of a metallic body, it immediately diffuses itself to an equality throughout the whole substance of the said body, and there results an increase in the density of the electrical fire, proportioned to the quantity that has been added, directly, and the capacity of the body, inversely. Likewise, if any quantity of fire be taken from a part of a metallic body, the remaining fire directly diffuses itself to all parts of the substance; and there results a diminution in the density of the fire, proportioned to the subtracted part, directly, and the capacity of the body, inversely. It is chiefly in consequence of such a propriety that metallic bodies are called *conductors*.

ductors or *deferent*; and indeed they are so to such a degree only as long as they actually keep their metallic form.

17. Next to metallic bodies, such liquids as are lean and not inflammable, are conductors, though in a much less perfect degree: and consequently animals, plants, which all abound with the said liquids, and other bodies that attract and retain the same, are capable of conducting likewise; and, indeed, the less these bodies have of moisture in themselves, the less they possess the power of conducting. Electroscopes may serve as an example, which in very dry weather move but very slowly, because the electrical fire can then have but a retarded motion through their dried fibres.

18. Hence, a mixture, whether natural or artificial, of humid particles, and also a mixture of metallic particles, renders bodies capable of conducting in any proportional degree. Walls, the ground, bricks, stones of a porous nature, are *deferent* only in consequence of either of those two causes, or of both. We shall see how lightning chooses out in its way some particular stones, only in consequence of their being of a particularly metallic nature.

19. However, it must be confessed, that there are some sorts of bodies which are in some degree *deferent*, and which, nevertheless, are not indebted for that their propriety to any moist or metallic particles they have within themselves: such undoubtedly are charcoal, and any glass recently taken from the furnace, as Dr. Priestley has demonstrated in his valuable History of Electricity, printed in London, &c. 1767.

20. In short, it appears, that in this case likewise, nature proceeds by a gradual series: and indeed there are bodies which from the greatest degree of *deferency* or *conductiveness*, gradually descend to the smallest; and the two extremities, of bodies perfectly *deferent*, and of bodies perfectly *insulating*, may be said to join.

21. Among *insulating* bodies, air, to the mortification of reasoners, and the no small trouble of experiment-makers, must be ranked first; which, however rare, actually stops the electrical fire, that runs with so much ease and celerity through the most compact metals. Air, therefore, is in its nature insulating; and it being every

every where plentifully diffused, supplies, in experiments, the greatest part of the *insulation*, since it is sufficient to suspend in the air with insulating strings, or raise with insulating supports, the conducting body, to have it completely insulated.

22. Therefore, since the electrical fire can run wherever it finds any moisture, air (and the same must be understood of all other *repressing* bodies) will be able to insulate, but only in proportion as it is dry. This is an observation of which I cannot remind too much those who might pretend to succeed in all seasons, and even in all places, in experiments of a delicate nature. I beg leave to introduce here an example: in the bottom of the electric *well* *, an insulated body contracts no electricity; but if I only breathe in the *well*, the same body will contract a degree of electricity.

23. Experiments, indeed, may be made in spite of the weather, in an air artificially warmed and dried; but if such an air remains entirely close, the transpiration of the persons in the room, independently of the vapours from the fire, will be enough to render it in some degree damp: if it circulates, then the heat will indeed lessen, but not annihilate the damp vapours. Consequently, such experiments only may be attempted as do not require an exact insulation, nor the utmost intensity of electricity.

24. Now to resume our enumeration; among bodies naturally insulating, I principally reckon the various species of sulphur, and generally all resins that are not soluble in water, and do not attract it, as other bodies do.

25. But for this same reason, and for their superior solidity, as well as figurability, I give the preference to woods that, with the help of art, are rendered capable of insulating, by being dried at several times in an oven, and at every time anointed with oil of linseed, so that there results from it a varnish close enough to preclude any entrance to exterior moisture. These I call *oiled woods*.

26. Besides, there are, if they might be kept free from dampness, several other bodies very useful for insulating. In order to insulate *by suspending* them, small cords of wool are very convenient, and much more those of the best sort of silk, which are not impreg-

* The experiment of the electric *well* is described in the paragraph, 442.

nated with salts that may attract dampness, as for instance are those dyed black.

27. In order to insulate bodies by *raising* them, the various sorts of crystal, and glass of all kinds, are made use of. I shall take this occasion to observe, that metals very possibly owe to their phlogistic their propriety of conducting, since by vitrifying them, that is, by taking from them their phlogistic, they become capable of insulating; and by bringing them back to their metallic form, that is, by rendering them their phlogistic, they are again made capable of conducting. But with regard to spirits, for instance the spirit of wine, it appears, that their phlogistic serves, on the contrary, to render them capable of insulating.

28. Precious stones can also insulate; but are not made use of on account of their smallness. The excremental parts of animals, hair, feathers, nails, horns, when freed from moisture, can likewise insulate, as well as the fibrous parts of animals and plants, which, as we observed before of wood, become capable of insulating whenever they are rendered sufficiently dry; and that they may constantly keep so, it will suffice to varnish them in any manner you please, as we have said of wood.

29. But this division between *insulating* and *conducting* bodies, will bring on another, which shews what sort of bodies are capable of *unbalancing* the electrical fire, and diffusing, if I am allowed the expression, its *inæquilibrium*; and, besides, in what manner they can effect this, which indeed is the principle of all the electrical phenomena. In short, the rubbing of insulating bodies, or bodies *electric per se*, against conducting or *non-electric* bodies, is, till now, almost the sole means that we know of transporting the electrical fire from one body into another, and putting it out of its natural æquilibrium.

30. I say, the almost sole means; because the precious stone known by the name of tourmalin, is electrified without any other operation but the heating it, and letting it cool. That sort of stone is formed of small long-shaped crystals, which, being united lengthways, make it terminate in a round shape, with rectangular sides, disposed in angles, some pointing outwards, others inwards. Those of this sort of gems which come from the East-Indies, and especially

cially from the isle of Ceylon, are of a brown dark red colour, which inclines to the colour of coffee: those which come from South America, are of the same colour with the ruby, or paler; sometimes of an orange colour, or even greenish.

31. Those gems are the only kinds of body that may be electrified, as far as experience informs us, by the sole increase or decrease of their heat. Therefore, though philosophers have generally asserted that sulphurs, resins, and mixtures made of them, may be electrified by heating, or melting them, and, if carefully kept, will retain for years their electricity, yet I think I have found the untruth of such an opinion, and the reason that has caused it to be universally received. This is, that the smallest possible friction is sufficient to excite a degree of electricity, in such bodies as have been carefully dried: in moving the vase in which they were melted, in taking them out of it, in unfolding the paper in which they were kept, or even in placing them upon a table, it is next to impossible to avoid any small friction, sufficient to lead into a mistake. But if you take one end of a stick of sealing-wax, of sulphur, &c. and heat it in any degree, and then let it cool, holding it all the while by the same end, and taking care not to handle it otherwise, or let it touch any other body, you will find yourself absolutely unable to draw with it the smallest hair, or thread; but whenever the stick has been in contact with any other body, it will immediately attract them.

32. It is true, that in cutting with a knife lumps of sulphur, or sealing-wax, in order to give them a round shape, I find that they become electrified; but I do not take this to be in the least a new way of exciting the electrical fire: a knife does not separate the prominent parts of a body, but by scraping and rubbing the inferior ones; therefore, in this case likewise, electricity is excited by friction.

33. Proceeding farther, I will say, that electricity is excited by the friction of an insulating, or *per se electric* body; especially when this friction is made with a *deferent* or conducting body. With regard to these we may observe, that two insulating bodies may very well, when rubbed against each other, excite electricity; but in order to render it continued, the insulating body with which
the

the friction is made, must have very near it a conducting body, incessantly to supply it with what fire the rubbed insulating body takes from it; or incessantly to receive and diffuse away the fire which the same communicates to it.

34. As to the friction of two conducting bodies with one another, it is an undoubted fact, that it cannot produce the least electricity. I have often, being placed upon an insulated stool, rubbed, scraped, or sawed balls or cubes of various metals, and there never resulted the least signs of electricity, as would undoubtedly have been the case, if electrical signs proceeded from either an absolute augmentation, or an absolute diminution of the electrical fire, and not from a simple translocation of the same, from one body to another. According to such an hypothesis, there would have resulted either an excess, or deficiency in the ball, or in the stool, or in my body; but the truth was, that the fire that was transported by any friction whatsoever, from the one to the other, instantly flowed back to its former place.

35. After all, it appears that insulating bodies, at least most of them, require to be rubbed with conducting bodies. I have immersed in some quicksilver contained in a deep, narrow, semicircular box of oiled wood, the plate of glass, which I now use in my apparatus instead of a globe, or cylinder; and having this plate turned round as I use to do, in that highly conducting fluid (which communicated with the floor through an iron wire) it afforded me a very strong electricity; to which I must add, that I could not move it but slowly, for fear of making the mercury leap out of the box.

36. This is an inconveniency to which the excellent rubbing amalgam, advised by Mr. Canton, in the 52d volume of the Philosophical Transactions, is not liable. Upon a silk cloth, covered with wax, a little amalgam must be spread, made of mercury and tin, powdered with chalk; upon this the friction is to be made: that cloth, thus prepared, is extremely useful for rubbing a tube of glass, or an oiled stick; when it is applied to cushions, it renders them extremely proper for rubbing the globes and plates, used in the ordinary complete apparatus, which I am now going to describe.

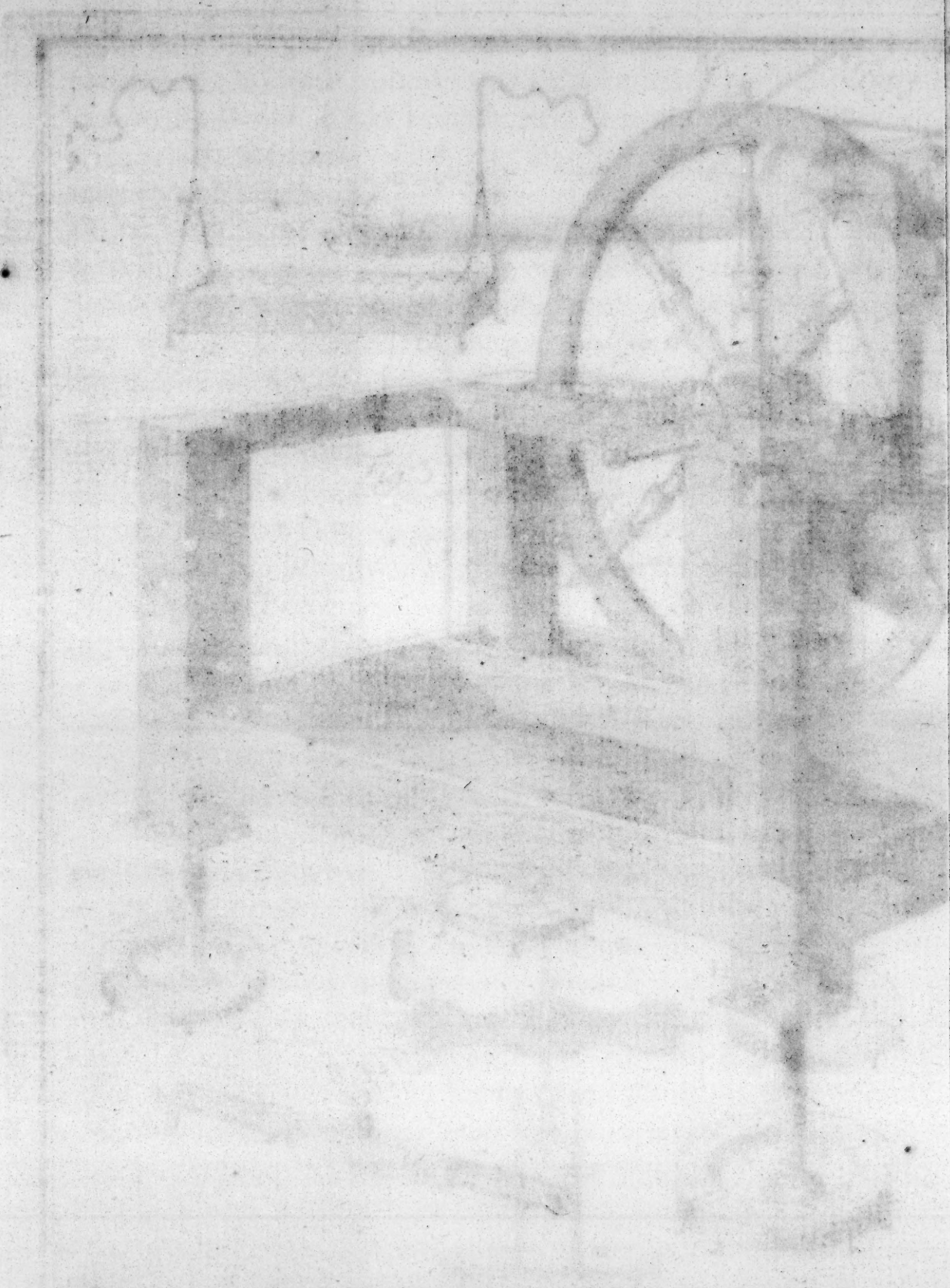
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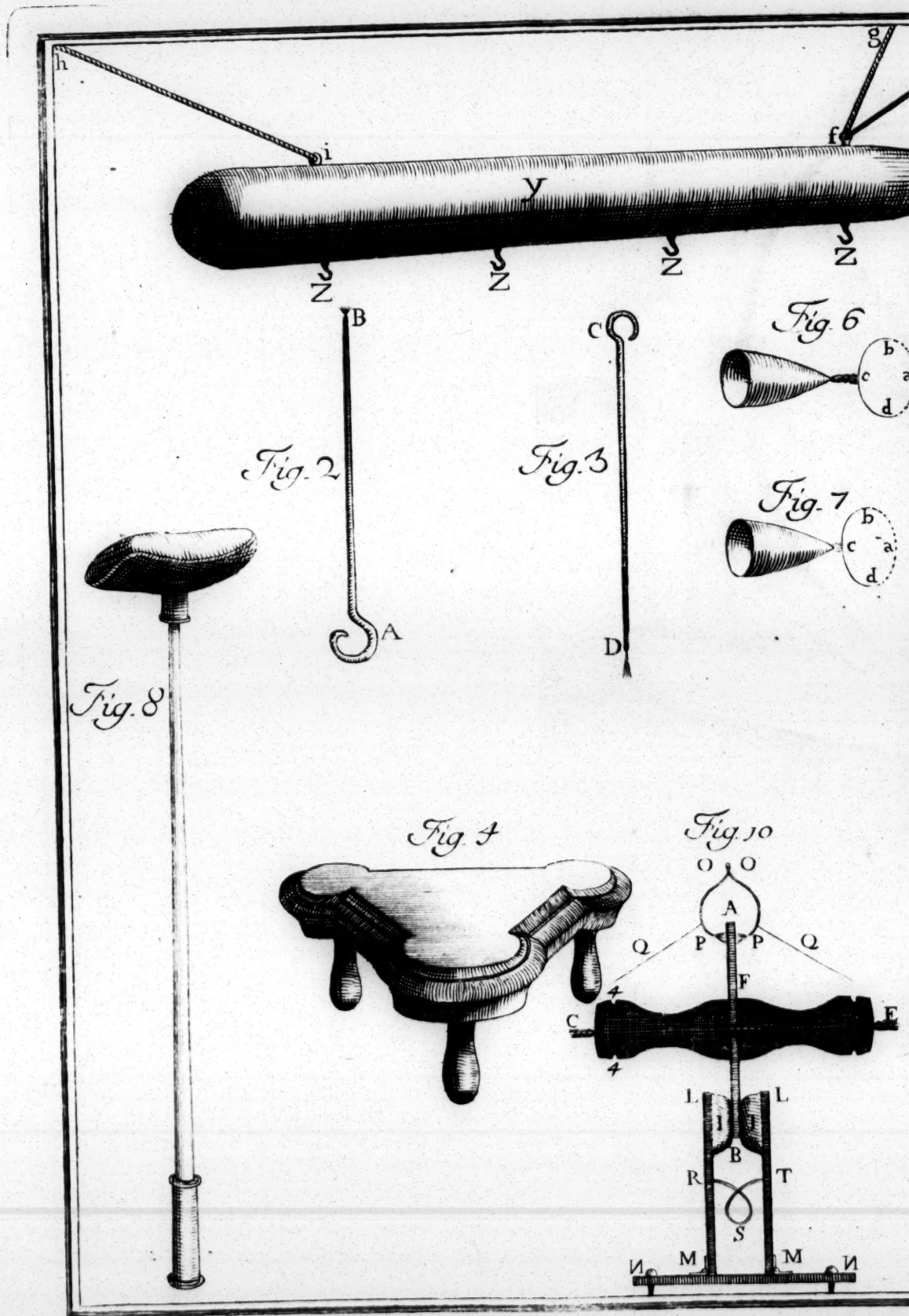
37. But

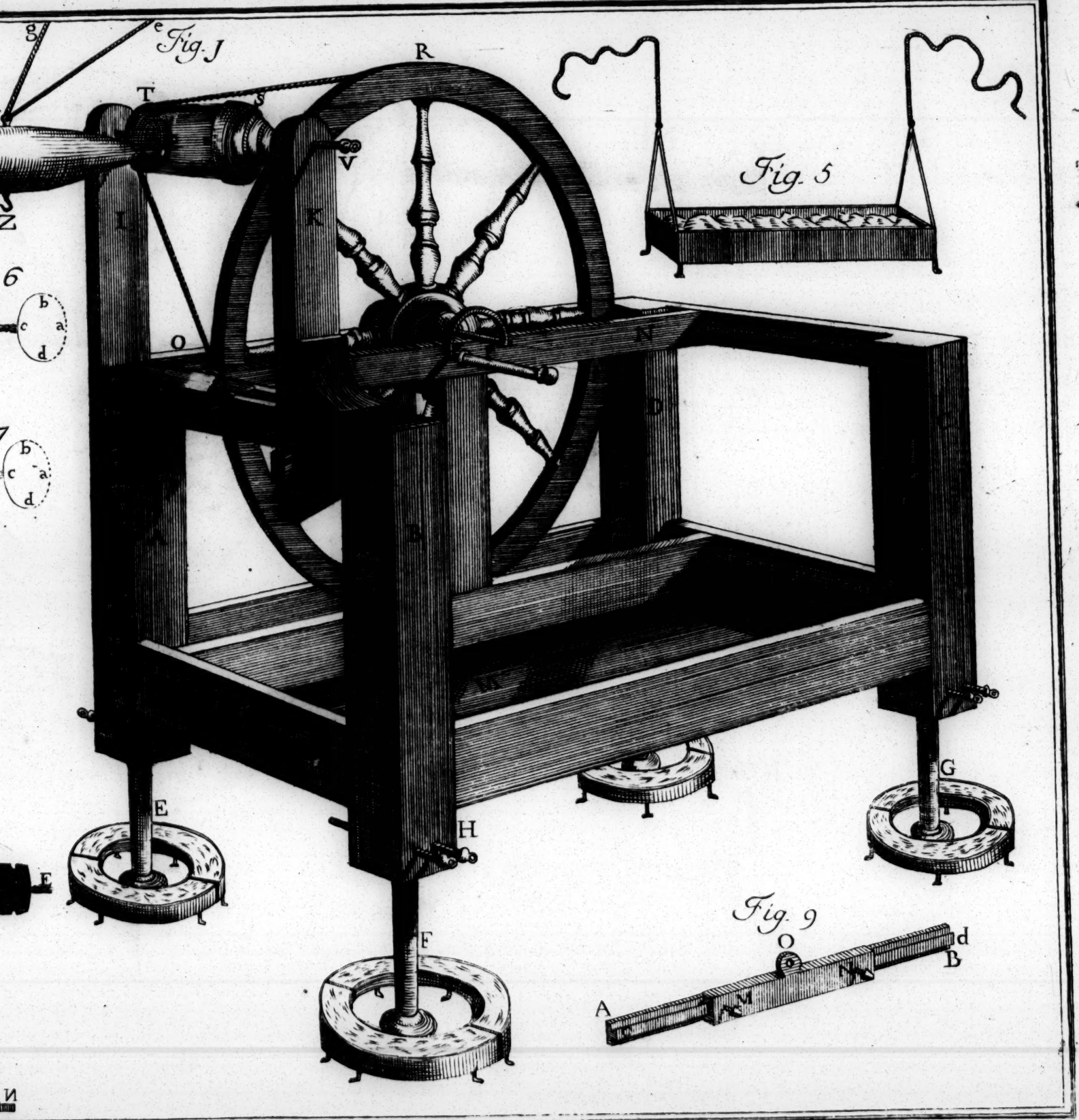
37. But I shall first give a summary idea of the apparatus itself, and of the theory of their functions, by relating an experiment which is the same with which Dr. Franklin originally explained the said theory. There are three persons A, B, C: the two persons A, B, stand insulated upon two stools, armed with supports of glass (Tab. I. fig. 4.) the person C stands upon the floor, in the middle of the two insulated persons. The person A rubs a tube of glass and presents it to the person B, who reciprocally presents to him a point. This is the whole of the operation through which the tube, and the two persons become electrified: let us now see which way this is effected. 1. The rubbed tube takes the fire from the rubbing person A. 2. The rubbed tube, presented to the person B, imparts to him the fire it has taken from the person A: and thus, there results an electricity *per deficiency*, or negative-electricity in A; and an electricity *per excess*, or positive electricity, in the person B. A can receive fire from C, who stands on the floor and has the same natural proportion of fire with it, or from B, who has a far greater proportion; B can impart some of his fire to C, who has but the natural quantity, or to A, who has still less.

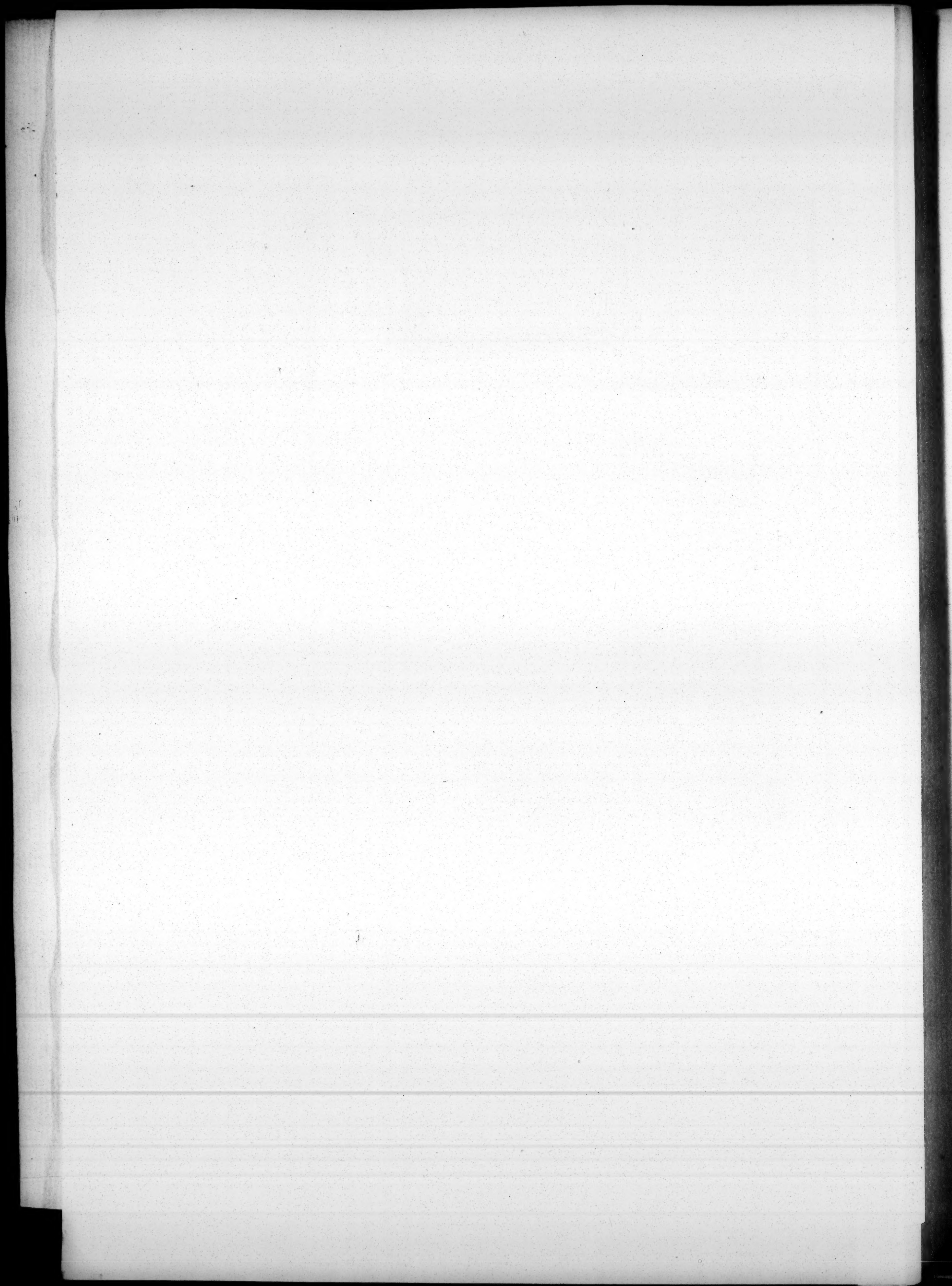
38. If the person A rubs an oiled stick, the stick will deposit its fire into the person A; and the same, being presented to the point held by the person B, will draw from him his natural fire: A will be electrified positively; B negatively.

39. But in such an experiment the tube will draw but little fire from the body of the electrified person A, when insulated; and therefore will also impart less of the same to the person B. Likewise the oiled stick will give but little fire to the person A; and consequently extract less from the person B. But if A comes to communicate with the person C, then the tube becomes enabled to draw a much greater quantity of fire from the much larger capacity of the floor; then likewise, the oiled stick will have room to diffuse away more of its own fire: hence the tube, or the stick, will be able to create in the person B a greater, either *excess*, or *deficiency*.









C H A P. II.

On the electrical apparatus.

40. **A**N insulating, or *per se electric* body, and two conducting bodies, therefore constitute an electrical apparatus: viz. an insulating body which is to be rubbed by a conducting body, and have a communication through its rubbed part with another conducting body; and two conducting bodies that may be insulated at pleasure, either one, or both of them. And such an apparatus will be the more perfect as its size and exactness taken together, are greater.

41. Here follows the description of the apparatus I make use of.
 1. In T S, between two poles an insulating body is to be fitted, which in the case of the figure Pl. I. Fig. 1. is a cylinder of glass.
 2. The frame, made of little beams, and boards, A B C D M N O I K, is destined to support, first the insulating body T S, then the wheel R, and lastly it must be able to receive in M, the man who turns the wheel, and him who rubs the cylinder. As these bodies, that is the frame of wood, and the man who turns the wheel, all communicate with the man who rubs, and may, all together, be insulated at once, they may be said to belong to the rubbing *conducting* body; and I comprehend their whole assemblage under the name of the *Machine*.
 3. Lastly, the hollow tube of brass Y, twelve feet long, and a foot in breadth, which has one of its ends shaped like a hemisphere, and with the other terminating in a conic point, stands in *c* near the equator of the rubbed cylinder, is the other deferent, or conducting body, in which the rubbed cylinder is to diffuse the electrical fire which it draws by the effect of the friction, (the property of glass is to draw fire) from the man that rubs, or from the machine, or through it, from the floor. This deferent body, of which the form and size may be varied at pleasure, is, indeed, usually called the *prime conductor*; however, whatever change may have been introduced since in the thing itself, I shall retain the old name of it, and call it the *Chain*; the first persons who made electrical experiments having commonly used a metallic chain.

42. This is the whole of the apparatus, let us now describe more exactly its parts: I shall begin with the machine. First of all, the materials that compose it must have a certain solidity, that it may be kept from being agitated and shaken by any motion, however quick of the wheel.

43. Secondly, the whole body of the machine, and the men who belong to it, must be able to be insulated, that experiments may be made on the negative electricity, which the friction of the glass will create in the aggregation of bodies that compose the machine when it is thus insulated. Supports, or little columns of solid glass, have appeared to me extremely fit for that purpose: they have but a small surface, which consequently can draw but little moisture, and the polish of the surface makes even such moisture to be very easily wiped off with a warm cloth: small columns of oiled wood would not be solid enough, or at least would shake. It only remains now strongly to unite the columns to the machine, and to the floor. In order to effect this, it is necessary first, to make the columns larger towards their head, which is to be inserted in the under part of the beams; secondly, the extremity of the beam must be sawed, first vertically, and then laterally, as may be seen in H, so that a part of the beam may be taken off. Thirdly, in the two surfaces of the beam, and of the piece taken from it that have been parted, two grooves must be formed, which, when joined, form a round hole, and are to receive the head of the column of glass: the latter must be garnished with leather, and when it has been inserted in the hole above, two strong screws (those in H are represented loose) drawn from the opposite part by their females screws, will fasten the whole with a great solidity. In order to prevent the columns moving about on the floor, they must be inserted in a little pedestal of wood, which by the help of three points of iron will be fixed in the floor. When I propose to make experiments, notwithstanding the unfavourableness of the weather, I used to surround each column with two little semicircular boxes of iron, which, when united around it, form a circle, and in these I put some hot cinders, in order to keep the column dry, without rendering the air *deferent*.

44. Thirdly,

44. Thirdly, the wheel must be so disposed as to be able to be brought nearer to the cylinder, or moved farther from the same, according as the rope will happen to be contracted in damp, or extended in dry weather. To that end . . . but all this will be better understood by a simple inspection of the plate (Pl. I. Fig. 1.) than by any explanation that might be given here; and any intelligent artificer will, of himself, hit upon the properest mechanical means to execute the whole with exactness.

45. Fourthly, the machine must carry the cylinder T S. (Pl. I. Fig. 1.) To that end, the two stout supporters I, K, are raised; they must be pretty high, to prevent the electricity of the cylinder T S, and of the chain Y, from flowing again into the machine*.

46. All these things above belong to that assemblage which we have called *the machine*; as likewise do the two armatures of wood, which are to be adapted in T and S, to the head of the cylinder, in order to settle it upon the poles, and the one of which is to receive around it the rope of the wheel. Of those armatures I shall speak as soon as I have explained what are the requisite qualities of the cylinder. The cylinder must be chosen round, thin, about nine or ten inches in diameter, and care must be had that it has been annealed. The roundness is necessary, to the end that when it has been well settled upon its pivots it may touch the rubbing substance with all parts of its surface. The thinness of the glass, which circumstance must not be to the detriment of the necessary solidity, makes the cylinder to be electrified with more facility and vivacity; the greatness of its diameter prevents its being heated so soon by the friction, because the same part does not turn again to the rubbing body (if the velocity of the revolutions be kept constant) but after intervals of time that are the longer, as the diameter of the cylinder is larger; the bigness of the cylinder having moreover this good effect that the chain may stand at a greater distance from the rubbing body; and thus the fire which it receives does not flow back into the latter. On the other hand, as the resistance arising from the friction increases as this is made at a greater distance from the centre of motion, and likewise, as cylinders of a larger size than ordinary, cannot have such an exact round shape, it is better to have

* For the properest manner of fastening them to the machine, see Plate I. Fig. 1.

them

them of a middling diameter. Lastly, with regard to cylinders or globes * made of a glass which has not been annealed, that is, which never passed through an almost insensible gradation from the heat of the furnace, to that of the open air, I shall observe that the want of a caution like this, has appeared to me always to be the cause of the breaking of globes or cylinders, (particularly if their thickness was somewhat greater than ordinary) and flying in shivers, to the danger of the assembly. I always considered those accidents as owing to a cause analogous to that which produces the rupture of the Bologna flasks, and of the *Batavic drops*: I think, that the friction actuates the principle which causes the breaking of cylinders or globes; which principle is no other than the fire in them, a great quantity of which is excluded from the exterior surface, then rapidly contracted and obstructed. Now, as glass which has been annealed is not subject to break, I thought that a similar caution would preserve either globes or cylinders; and, in fact, let the reason of it be what it will, none of those I have got to be thus prepared, has happened to break in my hands.

47. Besides the danger of having the cylinder broken on account of the rawness of the glass, there is another danger which is no less, that of having it leap out of the pivots on which it revolves. To that end, care has been taken that the supports I K should be strong, in order to prevent their receding from each other; the screws likewise have been made thick, and are immovably fastened with female screws: this done, the only remaining care is to insert the point of those screws into scutcheons of metal, out of which no agitation whatsoever may cause them to escape. Such scutcheons are to be inserted in the *armatures* of wood which are fitted to the heads of the cylinder or globe, and are besides fastened to the said armatures with three small screws, fixed through three little tails which come out of the body of the scutcheon.

48. What must be the shape of the said armatures, and how they are to be united to the cylinder, is scarce worth saying. They must be made so as to embrace a pretty large portion of the head of

* I prefer cylinders only because they are usually of a more exact round shape, and may at the same time be procured long enough to allow the rubbed *zone* to remain at a sufficient distance from the armature of wood fitted to them.

the cylinder, or globe; either of them, or both of them, must besides have one or more deep grooves cut into them, which are to receive the rope of the wheel. When I intend to fit such armatures to the globe or cylinder, I heat them, as well as the heads of the latter; within their cavity I pour some melted cement; then I apply them to the warm heads of the cylinder, or globe, which I fasten, thus armed, between their screws, or pivots. A rule placed between the two supports I K assists me in fixing the armatures in such a manner as to make the cylinder revolve with exactness, and touch, with all the parts of its rubbed zone, the body that performs the friction.

49. However, in all that I have hitherto said, concerning the choice of either globes or cylinders, and the best way of using them, my only inducement has been, that I looked upon them, notwithstanding the inferior quality of their glass, as being useful to those who cannot procure the apparatus of a plate of crystal, which apparatus is now used to a much greater advantage than globes or cylinders. I now make use of a plate which is about a foot in diameter, as in A B (Pl. I. Fig. 10.) it is polished like a looking glass, and very even, which circumstance may easily be discovered with a rule, or with a stretched thread. The said plate is cut round, and bored in the centre, so that the screw D, which comes out from the wooden support C D, may pass through it, and be inserted in the female screw bored in the other support F E G. These two supports are armed like the cylinder, with their scutcheons of metal, and are settled in lieu of it upon the same pivots, and then the rope of the wheel is set around the groove 4, 4, of the wheel R. In O P and O P are two rods of brass, which from the head of the chain present their points to the edges of the plate, and are fastened with two silk strings to the tops of the supports I K.

50. I commonly rub the plate by taking it between the index and the thumb of one of my hands, or between the most fleshy parts of both my hands, which, when they happen to be somewhat damp, prove excellent cushions; a little moisture being almost as efficacious as the amalgam.

51. However, when I propose to continue the friction for a long while, in which case the plate contracts much heat, then I make
use

use of the cushions I, I. (Pl. I. Fig. 10.) made of Turkey leather, stuffed with bits of cloth, and well rubbed with the amalgam. They are fastened on two little boards of oiled wood, L M, L M, and from the said cushions proceed several wires of iron which join the bodies from which I intend to draw electrical fire: this apparatus will be found very convenient for charging glasses. The two little boards L M, L M, are inserted into the table N N, which is fastened with screws to the beam Q. (Fig. 1.) The spring R, serves to make the cushions press gently against the plate.

52. Whenever globes or cylinders have contracted any considerable dampness, it is impossible to prevent the fire from diffusing itself from the rubbed zone, into the lateral armatures; and if the dampness lies in the inside of the globe, it is no easy task to take it out; but a plate, on the contrary, is very easily dried: besides, by its being rubbed on both its surfaces, it furnishes a double quantity of electric fire, though we must observe that the resistance from the friction becomes likewise double. There are some persons who use to rub the plate on two opposite parts of it, with two pair of cushions; but this contrivance produces the two bad effects of doubling the resistance arising from the revolving of the plate, and of heating it much sooner. Other persons adjust to one common axis, two, three, or even four plates of glass; and to each of them separate cushions. Surely, if no inconveniency arose from employing such a superior force as becomes necessary for surmounting the resistance from those multiplied frictions, it cannot be disputed but a given sum of electricity will thus be obtained in a time that will be proportionably shorter; or a much larger sum of electricity in a given time. On the other hand, however, it will be difficult to make every one of those many plates retain so well its fire as one will do; and it will be enough that one of them be not exactly insulated, to have the effect of all vanish to nothing; besides it will be likewise very difficult to fit the rubbing bodies with a same degree of exactness to every one of the plates as to one alone.

53. Many persons reduce those larger apparatus of one or more plates of glass, to two supports that come out of a piece of board; upon these two supports they settle the axis of one or more plates, which revolve by the help of a handle; and they adjust upon
this

upon that same piece of boards the two cushions, and fasten it with screws to a table. To say the truth, those machines of so small a bulk are extremely convenient, and easy to be transported about; but it is to be wished they might be equally serviceable and really good. Without mentioning the difficulty of finding tables solid enough to fasten upon them those apparatus, and that may be kept firm and free from agitation, notwithstanding the motion of the latter; omitting likewise the inequality from the interrupted motion of the handle, I shall only observe, that in order to obtain the greatest necessary effect, a velocity is requisite much superior to any one that a handle is able to communicate with a constancy: I own, that when the plate moves with a degree of velocity that exceeds certain limits, then the greatest part of its margin flies from the contact of the cushions, which only can touch it in the more prominent parts of its surface [it is impossible to find and adjust a plate that may have all the parts of its surface within the same plan] and have not time to close again towards the more depressed parts, and to press them; but, on the other hand, we must consider, that whenever the plate leaves the cushions and ceases to be in contact with them, a part of the fire gathered upon its surface always gets back into them (as we shall presently see), and that portion of fire which flows back thus, is the larger as the plate moves slower from the cushions; consequently a common handle gives a velocity inferior to that which is requisite for exciting either in the Chain, or the Machine, the greatest effect of the electricity: all this is moreover conformable to experience, which is enough for our purpose, let the reason of it be what it will.

54. With regard to the Chain, or prime conductor, I have nothing here to add, except that it must be of an even polished surface, that it may lose the less its electricity. I shall add that I make use of a conductor, and Machine (in this I comprehend, as has been observed before, all the several pieces that belong to it) of a considerable surface, in order to render sensible in public the several signs of their contrary electricity. Lastly, with regard to the insulating of the chain, or prime conductor, I use strings of organzine, disposed as in *fe, fg, ih*, (Pl. I. Fig. 1.) and when I am to make experiments in uncertain weather, I fit to each of the strings a little box of iron, filled with hot cinders.

C H A P. III.

In which is shewn the truth of the Franklinian theory, from the universal conformity of the same with the extensive laws of unity, existence, non-existence, and connection, to which the electrical signs, in the ordinary apparatus, are subject, as experience demonstrates.

55. **T**HE essence of the Franklinian theory is this. 1st, The electrical fire is diffused in all bodies universally, and balanced there with itself. 2dly, And universally. If the natural proportion of electrical fire in bodies becomes altered, this fire spreads itself from the bodies in which it is thus become *superabounding*, into those bodies where it is become *wanting*, with a force proportioned both to its relative excess, and to its absolute density; and in such passage through the resisting medium, it produces electrical signs. 3dly, And in the ordinary apparatus, the glass, when rubbed, draws from the rubbing Machine its natural electrical fire; and in revolving, carries it to the chain, into which it diffuses it, at that place to which it passes nearest.

56. I now propose to shew that the experiments made with the ordinary apparatus, in whatever manner they may be made, and combined together, all agree with the said theory above, as do also the general consequences or laws resulting from them; nay, manifestly require and claim this theory. And without loss of time, I proceed to enumerate those general and experimental laws, which, though they might be reduced to a less number, I shall, however, as I prefer clearness to brevity, carry to the number of seven. The first, I call it the law of *unity*; the second, the law of *non-existence*; the third, the law of *existence*; the fourth, I shall name it the law of *connection*; I shall give the appellation of law of *proportioned distribution* to the fifth; of *indication* to the sixth; and lastly, I shall design the seventh by the name of law of *excitation*.

57. And in order successively to demonstrate the universality of all these laws, I shall begin with that of *unity*; that is, I shall begin with showing that *all the vivid signs of electricity are produced by a same identical electrical fire, which exerts itself after a same constant manner.*

58. In

58. In fact, a given quantity of electricity, either in the Chain, or the Machine, is indifferently lessened or annihilated, whether by sparks, or by motions, or by a wind; or partly by sparks and partly by motions; or partly by motions and partly by a wind; or partly by a wind and partly by sparks; or wholly by sparks, or wholly by motions, or wholly by a wind.

59. In every one of those signs the electrical fire may be said really to manifest itself. If, standing in the dark, I try to lessen a given quantity of electricity by the means of a wind, I then see the electrical fire sparkle upon the point from which the wind blows, under the shape of a *brush*, or a *little star*. If I try to lessen the electricity by the help of the *motion* of a light suspended body, I see a little spark shine between the Chain or the Machine, and the little pendulum; or between the little pendulum and my finger.

60. However, in order to annihilate or suppress a given quantity of electricity, by the means of signs of a given kind, a greater number of these is required in proportion as a less quantity of the electrical fire is actuated and manifested in every one of them. Thus, in order to suppress a given electricity, a single spark, which I excite by presenting my finger to the electrified body, may happen to be sufficient, while one hundred vibrations of a small pendulum, which will excite perhaps one hundred sparks, small and continually decreasing, will prove insufficient. The wind will lessen the same electricity with a much greater celerity; but at the same time a light will take place, much superior to that excited by the pendulum.

61. All that we say here with respect to the destroying or lessening a given electricity, obtains also, as we shall see after, with respect to producing the same; that is, we shall see that an electricity may be produced by all the abovementioned signs, indifferently; and that a greater number of these signs will be required, as a less quantity of electrical fire will happen to be manifested in them. Whence it plainly appears that such signs may, in all cases, be substituted to each other indifferently, in order either to produce or destroy a given quantity of electricity; and that, whatever may be the kind of the sign which is made use of, a given sum of motion in the electric fire must take place, for producing the desired effect.

effect. Therefore, we must conclude that electrical signs are, in all cases, produced by an identical electrical fire, moving itself in a same constant manner; and in this consists the first law of unity. The abovementioned signs are likewise subject to the general laws expressed above, of *non-existence*, of *existence*, *connection*, &c. For the explanation of which laws we have prepared the way, in beginning with the first.

62. To proceed therefore to the law of *non-existence*; the first part of that law is, that *there never are electrical signs between two parts of one same system*, whether such system be an *indifferent system*, or one of the two systems which I call *animated systems*. In the ordinary apparatus, I call the glass an *animating system*, because it draws, when rubbed, the fire from the Machine, and, in revolving, carries it to the Chain, to which it imparts it: on account of those functions it therefore may be said to *animate* the electricity in the Machine negatively, and in the Chain positively: I reserve for the conclusion to discuss this system, and shew in what manner it performs its functions. On the other hand, I call *animated systems* the machine, and the chain: as to the floor, or bodies that communicate with it, I comprehend them under the appellation of the *strange* or *indifferent system*.

63. That between two parts of such indifferent system, no electrical signs ever are perceived, is a truth founded on constant experience: a spark never was seen to rise, or a light to appear, between the fingers of two men who, communicating with the ground, shook hands together. Such a *non-existence* of signs is a necessary consequence of the theory above: the electrical fire cannot be diffused in *indifferent* bodies without being diffused equally; or, at least, in proportion with the respective electrical capacities of these bodies if they are *deferent*, and communicate among themselves.

64. Hence, persons little used to electrical experiments do not fail to be astonished when they see two men, both insulated and communicating with the electrified Chain, excite strong sparks whenever they approach their fingers to some strange body, while they are unable to excite the smallest one by presenting them to each other: or when they see them agitate such corpuscula as are presented to them by strange bodies, and be absolutely unable to

communicate any such agitation to any one they may offer to each other.

65. Indeed this *non-existence* of signs between the various parts of a same system after whatever manner it may have been *animated*, follows likewise from the same theory: there is no reason why the electrical fire should pass from one part of the chain to the other, while, through the *deferency* of the substance of the latter, this fire is equally diffused within all its parts; and if superabounding in one, is necessarily balanced by an equal superabundance in all the others. The same may be said of the machine, however wanting or defective in electrical fire it may be; the fire remaining in one part is balanced by that which also remains in the other.

66. It is true, that if we use as a Chain, a body made of parts imperfectly united, viz. a real chain of iron, especially if rusty, we shall see when this chain is electrified, or when sparks are drawn from it, other sparks leap between the links, where they touch each other imperfectly and in few points; but this observation only serves to shew that the superabounding fire, which in the said chain proceeds from the globe, endeavours, conformably to the theory, equally to spread itself; and when effecting this, sparkles at every little resistance it finds in its way, and consequently at every separation between the links: this observation does by no means confirm an opinion, that the fire which is equally diffused in a body will not move from one part of it to the other. However, in order to avoid this seeming irregularity, I use as a Chain a conductor of a perfectly continued surface. Likewise, I use for Machines an aggregation of bodies, as continued and as even as can be procured, and all sufficiently deferent. I used at first to have the wood of the machine gilt; but I have since found it better to clothe it, at least the more outward parts, with tin.

67. It must be added, that from the same second principle, viz. that signs do not exist between two parts of a same system, it also follows that these signs will not exist, neither between an *animated* system, if it is not insulated, and strange bodies. The reason is, that the animated system will, in that case, be itself one of the strange bodies. Thus if the Chain communicates with the floor, a certain quantity of fire will indeed diffuse itself into it from the
glass;

glass; but then an equal quantity of it will run from the other extremity of the Chain into the floor. Likewise, a quantity of fire equal to that drawn by the glass from the machine, will be supplied from the floor into the same, through that part where their mutual communication takes place; thence it follows that the natural quantity of fire continues in the said systems, though electrified, provided they are not insulated. We might, if it were necessary, confirm that explication, by making those systems communicate less perfectly with the floor, for instance through a well dried flaxen thread; and in such case, we would see signs arise from such obstacles as the excessive fire would then meet in its way from the Chain into the floor; or the *natural* fire in its way from the floor into the Machine, which, when the electricity is excited with glass, is in a deficient state. Signs, indeed, will be perceived between the Chain not insulated, and the Machine if insulated; or between the Machine not insulated, and the Chain if insulated; but, in such cases, either the Chain, or the Machine, must be looked upon as being a part of the floor; consequently, such signs must be considered as signs between the *animated* insulated body, and the *indifferent* system.

68. The other part of the law of *non-existence* is that, *if two animated systems, that is the Machine and the Chain, communicate with each other, they manifest no electrical signs, neither between themselves, nor with regard to strange bodies, however insulated these two animated systems may be.* The reason is, that in such case the rubbed glass will indeed draw some electrical fire from the Machine, and transport it into the Chain, but as that fire is enabled, in consequence of the communication, to run back from the Chain into the Machine, it can neither become deficient in the one, nor *excessive* in the other.

69. Let us proceed now to the explanation of the third law, which I have named the law of *existence*. The first part of it is, *that if the Chain be insulated, and the Machine at the same time communicates with the floor, perpetual electrical signs will take place in the Chain.* Because, in such case, the fire which the glass draws from the Machine, is allowed to accumulate itself to a certain degree in the Chain; and as the Machine from which the glass draws its fire
by

by its communicating with the ground, becomes in a manner a part of it, and has consequently an infinite electrical capacity, the glass will continually be supplied with new fire from it: therefore any strange body that has only its natural quantity of electrical fire, will be able continually to draw fire from the Chain. Though I say continually, this must however be understood with the reserve that such continual extraction of the fire does not actually destroy the accumulation of it, or the insulation necessary to it. If I keep, for instance, my finger at the distance of two thirds of an inch, or an inch from the chain, I shall obtain sparks from it without end; that is to say, as long as the rubbing of the glass continues, and neither heat, or other accident disables it from performing its function. Those sparks, however, will be separated from each other by such an interval of time as will be requisite to enable the glass to transport into the conductor, the *excess* of fire necessary for surmounting the resisting space of either the two thirds of an inch, or of an inch. If after bringing your finger to a contact with the Chain, and keeping it united to it, you pretend to obtain farther signs, it will be a vain pretension, since you have destroyed beforehand the necessary condition of the existence of signs, that is the insulation.

70. It will not be thought improper here to observe that we may compare the strength of different apparatus, by holding a given strange body at a given distance from similar parts of these apparatus, as I have done just now with my finger. According as the number of the sparks that issue in a given time from a given conductor, is greater, the fire supplied by the glass must be concluded to be more copious, and the apparatus to which it belongs more excellent.

71. I must add, that from the preceding observation we may infer which is the best way to obtain the greatest signs, that is, the greatest sparks, from a given conductor. When a spark has been drawn, the body which drew it must be removed, and not approach again till the conductor be charged afresh to the greatest possible excess it is capable to receive.

72. The other part of the law of *existence* regards the Machine, and resembles the first: that is, *if the Machine is insulated, and the Chain*

Chain communicates with the ground, there will be perpetual signs between the ground and the Machine; the reason is, because a same quantity as that which the glass draws from the Machine, will be diffused through the Chain, which communicates with the ground, into the infinite capacity of the latter: consequently, no fire will be accumulated and forced back into the Machine or the glass, and prevent the latter from extracting fresh fire from the Machine; so that the glass will in such case be able to extract whatever fire may remain in the Machine, and whatever quantity may at due intervals of time be diffused into it (it being in a wanting state) from any strange body that has the natural proportion of electrical fire.

73. Those two first parts of the law of *existence* supposed that the one of the animated systems communicated with the ground, and from that hypothesis there must result a perpetuity of signs between the other animated system and the ground; now the third part of this same law will suppose that both animated systems are insulated, and is subdivided into two other parts, according as the signs are excited between the said systems, or either of them and strange bodies.

74. Therefore, with regard to the first case I say, *that if the Machine and the Chain be both of them insulated, there will be between them perpetual signs.* I insulate myself, and with one of my hands I touch constantly the insulated Chain; at distant intervals of time I then present my other hand to a person who touches the insulated Machine; and the result is, that I am continually giving him sparks after sparks. This is because every spark that I give is, in fact, a little sum of fire, which the glass can extract anew from the machine; the same fire can anew be accumulated into the Chain of which I am a part; and I can diffuse it anew into the Machine, which also is exhausted anew.

75. Let us retain the same hypothesis, and continue to suppose that the Chain and the Machine are both insulated; and instead of examining, as in the preceding case, the signs that take place between those systems, let us consider the signs that may arise between one of them, and strange bodies. But here arises another subdivision, that is to say, the Machine and the Chain either are tried together, or are not: if it be the first case, I say, that *there will*

will be perpetual signs between the strange bodies and the Chain, and between the strange bodies and the Machine. In fact, if while I try the Chain with one hand, and in consequence draw from it the fire that the glass has transported into it from the Machine, I at the same time try the Machine with my other hand, I then return to the latter the fire of which the glass has robbed it: I also give to it a quantity of fire; which quantity the glass will again take from it and diffuse into the Chain: I then shall always have an excess to take from the Chain, or a deficiency to supply in the Machine.

76. But if the Machine and Chain continuing insulated, I try only one of these two systems, *I then shall only obtain very few signs, and these will be proportioned to the capacity of the other system.* Thus, if I determine to try the Chain alone, I shall only obtain one, two, or at most three sparks, and those continually decreasing; because I shall then be able to extract from the insulated Chain but as much fire as the glass shall have been able to transport from the Machine into it. Likewise, if I determine to try the Machine alone, I shall then produce only some little decreasing sparks; because in this case I shall not be able to send into it but just a quantity of fire equal to that which the glass shall have been able to take from it and carry to the Chain.

77. Farther, if after having exhausted the electricity of one of the animated systems, I pass to the other and try it, *I shall have the same signs, still decreasing, but then a greater sum of them, that is, a sum proportioned to the capacity of the two systems taken together.* Thus, if after having exhausted first the electricity of the insulated Chain, I pass to the Machine, I shall then be able to give to it a quantity of fire equal, both to that which the glass can again transport from it into the Chain, and to that which is necessary to make up the deficiency introduced before into it. Likewise, if after having destroyed first the electricity of the Machine, I pass to the Chain, I shall find myself able to draw from it a quantity of fire equal both to the excess already introduced in it, and to the deficiency which the glass can again create in the Machine. Lastly, if both systems are alternately tried, by the same reason, signs will be alternately obtained, which will first decrease, then fail entirely.

78. In repeating these experiments I have sought to increase considerably the capacity of either, or both of the animated systems, by adding to them a series of insulated men; and I have always seen the consequence to be, that the signs from the one of the systems always increased with the increase of the capacity of the other; and when I pursued the experiment, that the signs from the other system increased with the increase of the capacity of both.

79. Indeed that law demonstrates beyond a doubt, that *in all bodies a certain quantity of electrical fire is naturally diffused; and that to any body whatever a certain quantity of electrical fire may be added.* A man A is insulated in such a situation that he may with one of his arms reach the insulated Machine; and with a rod of brass which he holds in the other, touch a pail of water, or any other body also insulated: this done, after having drawn from the Chain all the sparks I can draw from it, I bid the man to touch the Machine; and the consequence is that I then can draw from the Chain another spark, and no more; which spark is no other than the fire contained in the man A, and which the glass can transport from him into the Chain, he being become a part of the Machine. I make again a sign to the man to touch now the pail of water with his rod; as the pail is then become a part of the Machine, I obtain from the Chain another spark, in consequence of the new fire which the glass has extracted from the water, or any other body that may have been used in its stead.

80. I dispose things so as to have the man A insulated in such a manner that he may touch on the one hand the insulated Chain, on the other the pail of water or any other insulated body; then I give to the Machine the fire that is necessary to make up its deficiency, and till it receives no more. But as soon as the man touches the Chain, then the Machine draws another spark and no more: when the man stretches the rod to the pail of water, the Machine becomes again able to receive from me another spark, all which I explain thus; when the man communicates with the Chain, the excess of it diffuses itself for a part into him; and when the man introduces likewise a communication between the Chain and the pail of water, the excess in both the Chain and the man, is again divided and diffused into the water; so that, in these two cases,

cases, the glass will be able to draw from the Machine, and transport into the Chain, a proportionally larger quantity of fire.

81. All these things, on which I have hitherto pretty much expatiated, indeed comprehend the whole substance of what I am going to say concerning the law that I have called *of connection*; but I think that it will not be deemed superfluous to confirm those truths by considering them under different relations, and examining them through other experiments. The law of connection is therefore, that *absolute electrical signs lessen absolute electrical signs of the same name, but increase absolute electrical signs of a contrary name; and that respective signs diminish both respective and absolute signs*. When I introduce the terms *absolute signs*, and *respective signs*, I have no other reason but to expose the law with less words; and by the words *absolute signs*, I mean those that are excited between the ground, and one of the animated systems: by *respective signs*, I mean those that are excited between the two animated systems. Again, I call *absolute signs of a same name*, those that are excited between the ground and the Chain, when they come to be compared with others excited in the same manner; or those excited between the ground and the Machine, when compared with those excited between the ground and the Machine. On the other hand, I call *signs of a contrary name*, those that are excited between the ground and the Chain, with respect to those excited between the ground and the Machine.

82. I express the whole of the law above by a single experiment: I adapt two very sensible electrosopes, the one to the Chain, the other to the Machine, which, as the law expresses it, must be both insulated. I say, two very sensible electrosopes, because if they are made as usual, of thread, though they are ever so mobile on account of their lightness, however in dry weather they stand almost without motion, and instead of moving towards each other when I lessen the electricity which they are destined to indicate, they continue to recede from each other: the reason of this is, that their electricity does not by any means keep pace in its decrease with the electricity in the Chain, owing to their being in a great degree insulated from the dryness of the weather; which circumstance makes them to manifest the decrease of the electri-

city either in the Chain or the Machine, but after an interval of time proportioned to the difficulty they find in dissipating their own. Therefore, in that case and others of the same kind, instead of the threads and balls, I use two stripes of tinfoil, two or three inches long, and two lines wide, which are fixed to a band of gilt paper, and thus hang contiguous and parallel to each other.

83. And to proceed to the experiment itself: 1st. I observe that as soon as I draw a spark from the Chain, the divergency of the metallic stripe that communicates with it lessens, and that of those annexed to the Machine is increased. 2. When I give sparks to the Machine, the stripes that communicate with it lose their divergence; those annexed to the Chain recover theirs: now, the manner of the divergence of the metallic stripes shews the kind of the electricity of a given system, or otherwise, what signs can be obtained from it? consequently, we may say that the signs between the Chain and the ground, lessen the signs between that same Chain and the ground, and increase those between the ground and the Machine: and, in the same manner, the signs between the Machine and the ground lessen those between that same Machine and the ground, and increase those between the ground and the Chain. The reason of all is evident, and follows from what has been said with respect to the laws expressed before: to draw signs from the Chain is to diminish its excess, and create in the said Chain a vacancy in which the glass may again transport a quantity of natural fire from the Machine; whence there results in the latter an additional deficiency. To create signs in the Machine, is the same as to supply it with a new fire, which the glass may again transport into the Chain, there to produce a new excess.

84. And, to speak the truth, we have with regard to destroying homologous signs by one another, more examples and confirmations than might be well wished for: when persons little conversant with electrical operations come to see our experiments, being in consequence the more curious and eager, they handle every thing, and take among themselves so much trouble that they at last see nothing.

85. With respect to *respective* signs, the truth of the law is no less beyond a doubt. I insulate myself and communicate with the Chain.

Chain; at every spark I then give to another person likewise insulated, and communicating with the Machine, the two electroscopes, that annexed to the Chain, and that annexed to the Machine, both fall down: a diminution therefore takes place then of the signs between the Chain and strange bodies, or between strange bodies and the Machine, that is to say, of the *absolute* signs; and there arises likewise a diminution of the signs between the Chain and the Machine, that is to say, of the *respective* signs. In fact, if in the same moment that I give the spark, the friction of the glass is interrupted, I cannot give a fresh spark, or if I still can, it will be less; it is, therefore, a matter of truth, that if we consider its function the glass may be said to begin again the circulation of the fire, but it is the *respective* signs that complete it.

C H A P. IV.

On the law of distribution.

86. **T**HOUGH this law is closely connected with the substance of those explained above, however, I will explain it separately in this chapter, as a thing that serves to prove, in a particular manner, the conformity of the theory with facts; a means, this, of attaining truth indispensable to philosophers, and the best guide they can have for recovering it when lost. The purport therefore of the law is, *that the sum of any sort of absolute electricity in any two bodies, distributes itself in them proportionably to their capacity, as soon as they come to communicate with each other.* I shall expose first all the particular cases belonging to that law; then I will proceed to shew, as far as it will relate to the subject, its general and exact conformity with experiments.

87. The natural density of the electric fire, as it is diffused in the ground, is the circumstance from which we form our calculations of the quantity of electricity in bodies. Bodies in which the electrical fire is equally dense with that in the ground, afford no sign of electricity; and those in which it has been rendered more or less dense afford signs, or at least are in a condition of procuring them, whenever they shall communicate with the ground.

88. There-

88. Therefore, any sort of *absolute* electricity is formed either by an excess of fire by which the fire of the given body is more dense than that in the ground, and is consequently called electricity by excess; or by a deficiency of the same, which makes the fire in the given body to be less dense than that in the ground, and is then called, electricity by deficiency. Therefore, if we multiply the excess of density in a given body relatively to that in the ground, by the capacity of the body itself, the produce will be the quantity of its electricity by excess, or also, the quantity by which the whole of its fire is denser than that in the ground: and likewise, if we multiply the deficiency of density in a given body, relatively to that of the fire in the ground, by the capacity of the said body, the produce will be the quantity of its electricity by deficiency, or the quantity of the deficiency by which it is less dense than the fire in the ground.

89. Hence if E expresses the excess of density of the fire in a given body, of the capacity A; A E will express the whole quantity of the excessive fire from which results the excess of density; and if S expresses the capacity of the ground, the following analogy will be the consequence of the abovementioned rule. $A + S : A E :: A : \frac{A A E}{A + S} :: S : \frac{S A E}{A + S}$; that is, as the sum of the capacities is to the whole excess, so are the capacities of the body, or of the ground, to the portions of the excess that belong to them: that is to say, the portion of excess that will remain in A, will be to that portion which will pass into the ground, as A : S. Now, as the capacity of the body A is infinitely small, relatively to the capacity of the ground, all the sum of the excess A E will pass from A into S, and during such passage, signs will be manifested, which will be proportioned to the whole above mentioned sum: therefore, that sum will express the precise quantity of the electricity.

90. Likewise if D expresses the deficiency of density in the fire of a given body of a capacity B, B D will express the whole quantity of fire that forms the deficiency of density in A. And in forming the same analogy as above, we shall see, that the portion of deficiency B D which will remain in the body, will be to the portion that will pass into the ground as B : S, that is to say, no part of
the

the deficiency BD will remain in the body B, all will pass into the ground, it being of an infinite capacity: or rather the ground will supply the body with what it is wanting of, so that the fire in it may be rendered of an equal density to that of the fire in the ground.

91. In short, when a body having any degree whatever of electricity, either per excess AE, or per deficiency BD, is made to communicate with the ground, all manner of electricity is suppressed; the whole excess, or the whole deficiency passes into the ground; tho' no perceivable electricity results in the ground from its receiving such finite quantities of electricity into its infinite capacity.

92. But the same does not hold true, when a communication is established between two particular bodies, which have, either one or both of them, any degree of absolute electricity. In this case, the fire that is distributed in both in an unequal proportion will, indeed, be brought to an equal density, and the electrical signs from the one to the other be suppressed; but their fire is not, for all that, brought to the same degree of density with the fire in the ground, nor is consequently their absolute electricity destroyed.

93. Therefore, that we may be able to discern in all cases the electrical signs that may take place between any two particular bodies whatever, as well as those between a particular body and the ground, it is requisite to divide not only the absolute, but likewise the respective kinds of electricity; that is, those which may exist in a particular body, relatively to another, and which consist of the quantity of fire that must pass from the one into the other for bringing them to a state of equal density. Those kinds of respective electricity will be, 1st, a respective electricity of simple excess, when a particular body A, having a degree of absolute electricity per excess, is made to communicate with another particular body that has no electricity. 2^{dly}, Respective electricity of simple deficiency, when a particular body A, having a degree of electricity by deficiency, is brought into a communication with another body B, that has no absolute electricity. 3^{dly}, Respective electricity of unequal excess, when the communication is established between two bodies, having both a degree of absolute electricity by excess, but those

those degrees are unequal. 4thly, Respective electricity of unequal deficiency, when the two bodies between which the communication is formed, are both electrified by deficiency, but in unequal degrees. 5thly, Lastly, respective electricity by excess and deficiency together, when the one of the two bodies that are brought to communicate together has an absolute electricity by excess, and the other by deficiency.

94. The degree, whatever it may be, of each of those kinds of respective electricity, is expressed in the following table, in which the quantity of fire that constitutes within the capacities A and B, their different density, is distributed in proportion to the said capacities; which quantity of fire, in cases of similar kinds of absolute electricity is equal to their difference; and in the case of contrary kinds of absolute electricity, is equal to their sum. E and D express the excess or deficiency of density in the fire. And e, d , will express another degree of excess or deficiency in the density.

	I.	II.	III.	IV.	V.	VI.
	As the sum of the capacities	is to the difference of the quantities of similar electricities, and to the quantities of contrary electricities.	So the capacity A	is to the difference or sum that remains in A	So the capacity B	is to the other portion that passes into B, and which constitutes the quantity of the respective electricity.
95.						
96.		AE		$\frac{AAE}{A+B}$		$\frac{BAE}{A+B}$
97.		AD		$\frac{AAD}{A+B}$		$\frac{BAD}{A+B}$
98.	$A+B$	$AE-Be$	$= A$	$\frac{AAE-ABe}{A+B}$	$= B$	$\frac{BAE-BBd}{A+B}$
99.		$AD-Bd$		$\frac{AAD-ABd}{A+B}$		$\frac{BAD-BBd}{A+B}$
100.		$AE+BD$		$\frac{AAE+ABD}{A+B}$		$\frac{BAE+BBd}{A+B}$

101. The quantity of a respective electricity in the case of a simple excess, or also of a simple deficiency, is less than that of an absolute electricity; that is to say, the quantity of the fire A E, or A D, that will pass from the body A into the ground, or from the ground into the body A, if this comes to communicate with it, is to the quantity $\frac{BAE}{A+B}$, or to the quantity $\frac{BAD}{A+B}$, when A comes to communicate with B, as the sum of the capacities A+B, is to the capacity B (VI. 96. 97.)

102. The respective electricity, in the case of an unequal excess, or of an unequal deficiency, is less than the respective electricity of simple excess, or of simple deficiency; since the former is to the latter, as the difference alone of the absolute electricities, (from which the former results) is to the whole of the absolute electricities from which the latter results.

103. But the respective electricity by excess and deficiency together, (the capacities are supposed constant) being proportioned to the sum of the absolute electricities (100.) will always be greater than the least absolute electricity, though always less than the greatest, whenever the absolute electricities will happen to be unequal; and will be equal to them, whenever they will happen to be equal. 1st, Let the absolute *excess* A E be equal to the absolute deficiency B D; if the former A E passes into the latter B D, or the former into the latter, which comes to the same, those parts of their values that will be equal and contrary, will destroy each other. 2dly, Let A E be greater than B D, there will first pass into B, as great a portion of A E as is necessary to destroy the whole B D; and that portion of respective electricity must be equal to the least absolute electricity; but, moreover, the overplus in A E above the destroyed quantity B D, will diffuse itself into A and B, proportionably to their respective capacities; in consequence of which diffusion, the respective electricity will indeed be less than the greatest absolute electricity A D, but greater than the least absolute one B D. 3dly, The same is to be said in the case where the absolute electricity A E should be less than the absolute one B D; the greatest respective electricity will be less than the greatest absolute one B D was, but greater than the least absolute one A E, by all the overplus in B D, above the value
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of A E; after the destroying of which quantity A E the said respective electricity will diffuse itself into the capacities A and B, proportionably to their respective amounts.

104. Whence it follows, that in the case of a respective electricity by excess and deficiency together, the absolute electricities either destroy each other, if they are equal, or the least one, in consequence of the communication between the two bodies, becomes of a contrary nature, and differs in that respect from the four other sorts of respective electricities; viz. those of simple excess, or deficiency, and of unequal excesses or deficiencies; in which respective electricities the remaining absolute ones both retain the same positive, or negative value of the primitive absolute electricities.

105. But let us now proceed to the experiments. Four men, A, B, C, D, are insulated in a proper position, each of them holds a very sensible electroscope between the index and thumb of his left hand; A touches the Chain with his right hand and then withdraws it; B touches the left hand of A with his right hand, which he then likewise withdraws; then C touches B, and lastly D touches C. In the same time that those several successive contacts are performed, the divergence of the electroscope held by the man who is touched, is lessened by a half part of it, and a divergence equal to the remaining part, rises in the electroscope of the man who has touched him. I afterwards touch from the floor, the men A, B, C, successively, when I draw from each of them sparks which prove successively diminished. Thus that experiment demonstrates, by the help of the regular diminution in the divergences and sparks, how the electricities of simple excess of different degrees, are diffused and distributed.

106. If the men A, B, C, stand with their respective degrees of electricity after the first operation, mentioned in the preceding paragraph, then A will retain $\frac{1}{2}$ (or $\frac{8}{16}$) B $\frac{1}{4}$ (or $\frac{4}{16}$) C $\frac{1}{8}$ (or $\frac{2}{16}$) of the whole electricity which A had drawn at first from the Chain; and D, who has not been touched by any other subsequent person, will have $\frac{3}{8}$ or $\frac{6}{16}$ of the same. Now, if in such a state of things, A touches B, he will give him a spark equal to $\frac{2}{16}$ of the former total electricity, and consequently shall retain $\frac{8}{16} - \frac{2}{16} = \text{to } \frac{4}{16} + \frac{2}{16}$, the value now in the man B. But if in the same former state of things,

A immediately touches C or D, instead of B, he will give to either of them a greater spark, which will be equal to $\frac{3}{16}$ of the original spark; thus $\frac{8}{16} - \frac{3}{16}$ will be equal to $\frac{5}{16} + \frac{3}{16}$. In this case, likewise, it will be convenient to use the electroscopes; and the agreement that will be observed between both the successive values of the sparks and the changes in the divergencies, on the one hand, and the reasoning that I have exposed above on the other, will demonstrate the truth of the theory of the electricity of unequal excess, from which that same reasoning is deduced. Nay, since such experiments, and others of a same kind may be repeated at pleasure, and be always attended with the same effects, (reserved the contrariety of the direction with regard to the Machine) they can likewise manifest how much respective electricity of simple excess, and electricity of unequal deficiency, has been introduced.

107. The Machine and Chain are insulated; the men A and B, are likewise insulated in a proper situation, and supplied with electroscopes; I touch the Machine, and the man B touches the Chain; then I leave the Machine and touch the Chain, and the man B touches the Machine. The electroscopes of A and B, diverge equally in consequence of equal but contrary electricities; the same friction of the glass being able to introduce from the ground into the man A, the same quantity of fire as that which it may extract, and diffuse into the ground from the man B, who is supposed of an equal capacity with the other. The man A touches the man B, and the electroscopes immediately lose all manner of divergency; which shews how absolute electricities, when equal and contrary, reciprocally destroy each other.

108. The man A stands insulated alone, and alternately touches the Machine and the Chain, which are insulated. At every successive contact the electroscope which he holds between his fingers falls down, and then rises and diverges again; which shews, that at every contact the electricity in the man A, turns into one of a contrary nature. The reason is, that the excess which the man A, who is of a capacity inferior to that of the Machine, draws from the Chain, is less than the deficiency that he receives from the Machine, which is, as we observed, of a superior capacity to his; and the deficiency that he may contract from his touching the

Machine is less than the excess he will receive from his touching the Chain, which is likewise of a capacity superior to his: which experiment will shew how the respective electricity per excess and deficiency together, transforms the least absolute electricity into the nature of the greatest absolute one.

109. If after the man A, in the preceding experiment, has touched the Chain, I touch both the Chain and the man, the spark from the man is less than that from the Chain: likewise, if after the man touched the Machine, I touch both the man and the Machine, the spark that I give to the man is less than that I give to the Machine. This proves how the transformed electricity remains less than the greatest transforming electricity.

110. But we must here observe, that at every touching of one of the animated systems, the divergency of the electroscope of the man becomes equal to the divergency of the fellow-electroscope which is annexed to the system; we may even lay it down in the most universal terms, that as soon as the quantities, whatever they are, of electricity, in two bodies of capacities however unequal, have been diffused into each other, by the means of a communication, or, as soon as the fire becomes diffused in the same bodies in an equal degree of density, whether positive or negative, the electroscopes always are found to have an equal divergence, let the quantity of the fire, either excessive or residual diffused in both be in what degree it will; which is owing to that fire being always diffused in these bodies proportionably to their capacities. Whence it follows universally, that the excessive or defective density of the fire is always manifested by the divergence of the electroscope; but the same cannot be universally said of the *quantity*. 2dly, The divergence manifests the quantity only when the bodies amongst which the fire has been diffused, are of an equal capacity, as was the case in the experiment of the four men insulated, who are to be supposed to be of an equal capacity; in which case the quantity, I say, is proportioned to the density.

111. Here a question can be proposed: in the respective electricity of unequal excess and deficiency, the same quantity of fire indeed passes from the one of the particular bodies into the other, as would do in the case of an absolute electricity; but will there be

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no difference in the manner after which the sparks will then be produced?—In comparing the sparks which, in the case of the experiment N^o 107, take place between the man A and the man B, it appeared to me that these sparks were more united, and leaped to a distance somewhat greater, and produced cracks sensibly louder, than those sparks did, which either of them gave or received from the floor: in both cases the same quantity of fire indeed passed, but the fire in the case of the respective electricity, seemed to me to have more impetuosity, and to be more compact. The same appeared to me still more evidently, when I compared the sparks between two men, who communicated, the one with the Chain, the other with the Machine and were constantly insulated, with the sparks that one of the men either gave or received from the floor. The same has been confirmed by Mons. Le Roy, in the volume of the *Memoires de l'Académie Royale des Sciences, de l'année 1753*, published in the year 1757, by exciting the sparks with a ball of brass, which, by the help of a rod annexed to it, was moved backwards and forwards within the inside of a tube of glass, of which the mouth rested on the body from which the said gentleman intended to draw the sparks. All this is conformable to the theory; a given sum of electrical fire must diffuse itself with more impetuosity from the Chain within which there is an excess of it, into the Machine where the same is deficient, than from the said Chain into the floor, which has the natural proportion of it. The fire must likewise have a greater impetuosity when diffusing itself from the floor, which has the natural proportion, into the Machine, which is deficient; all this will be proved by most evident experiments in the next chapter.

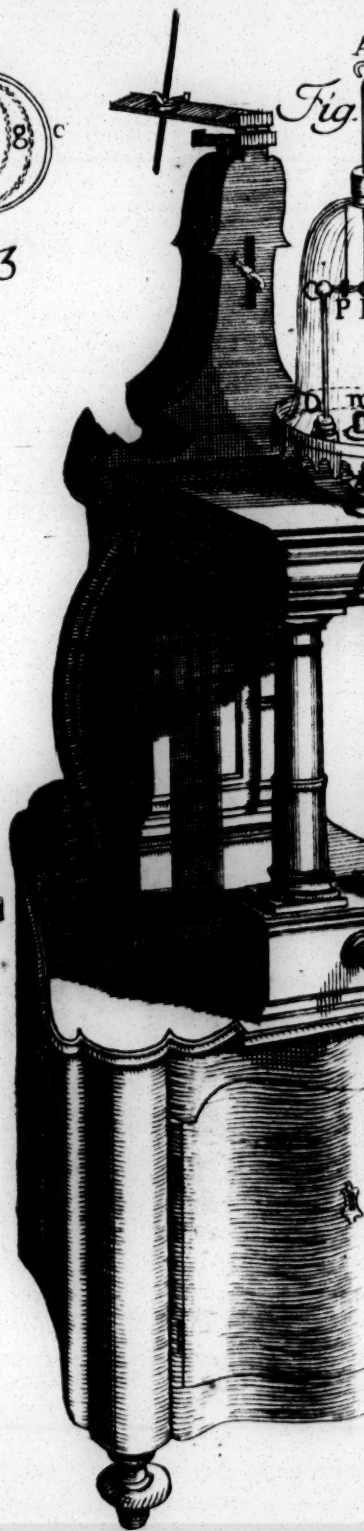
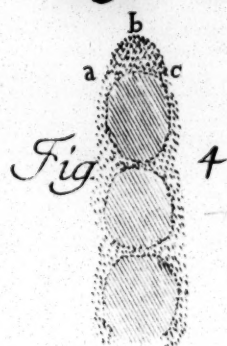
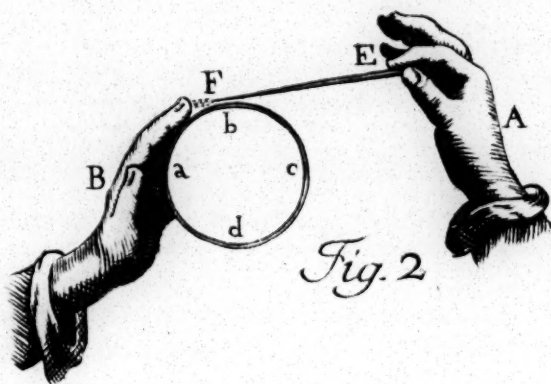
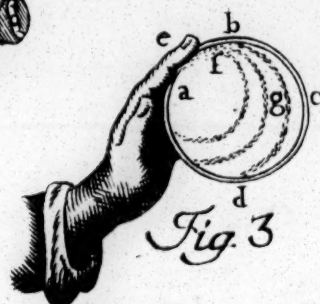
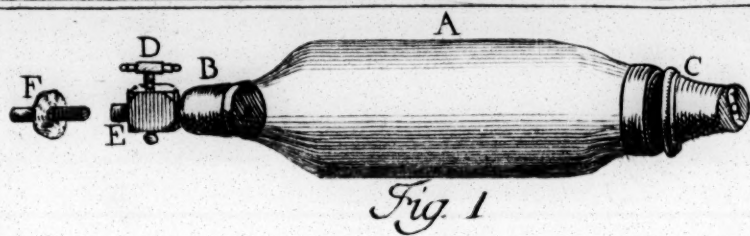
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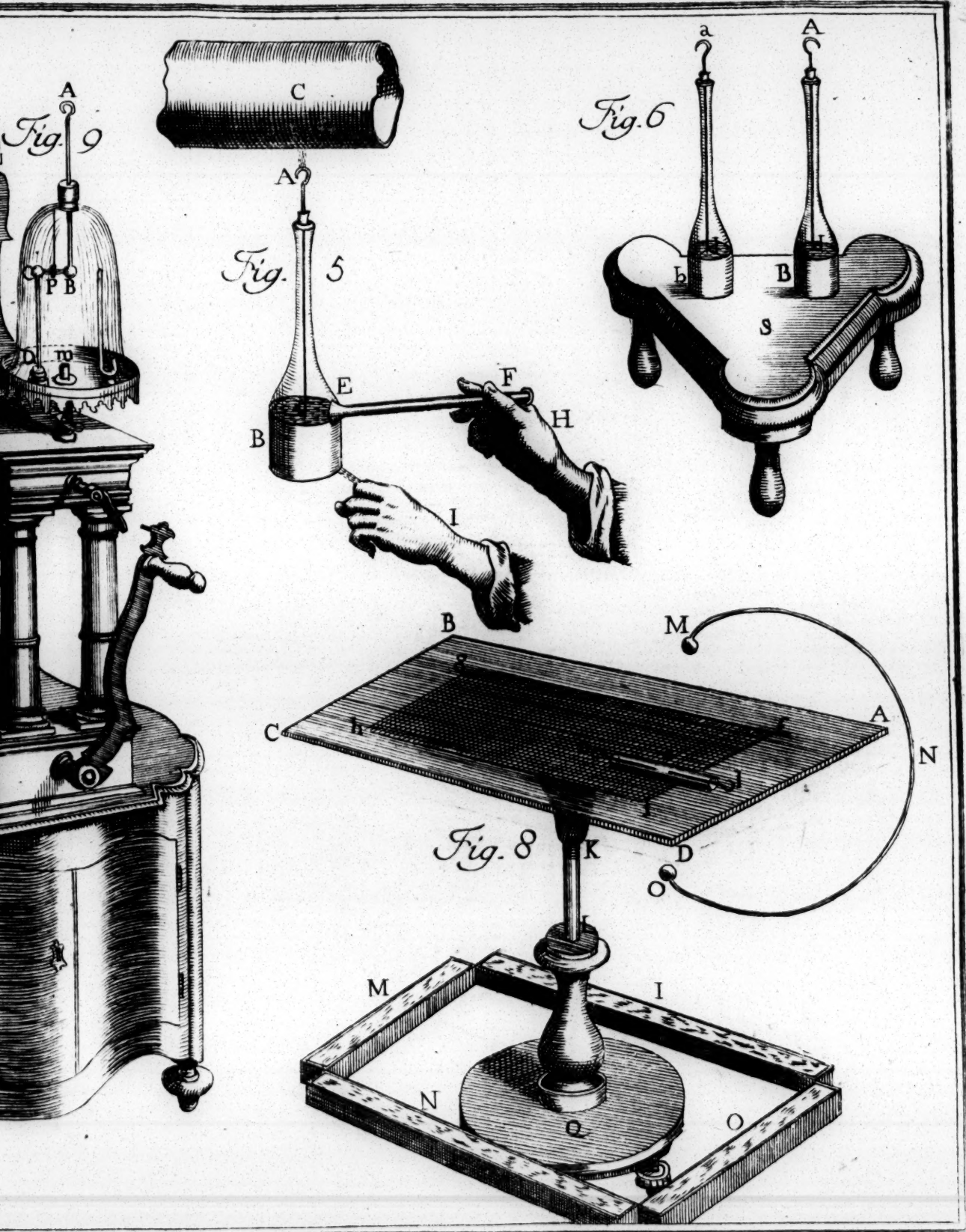
On the law of indication.

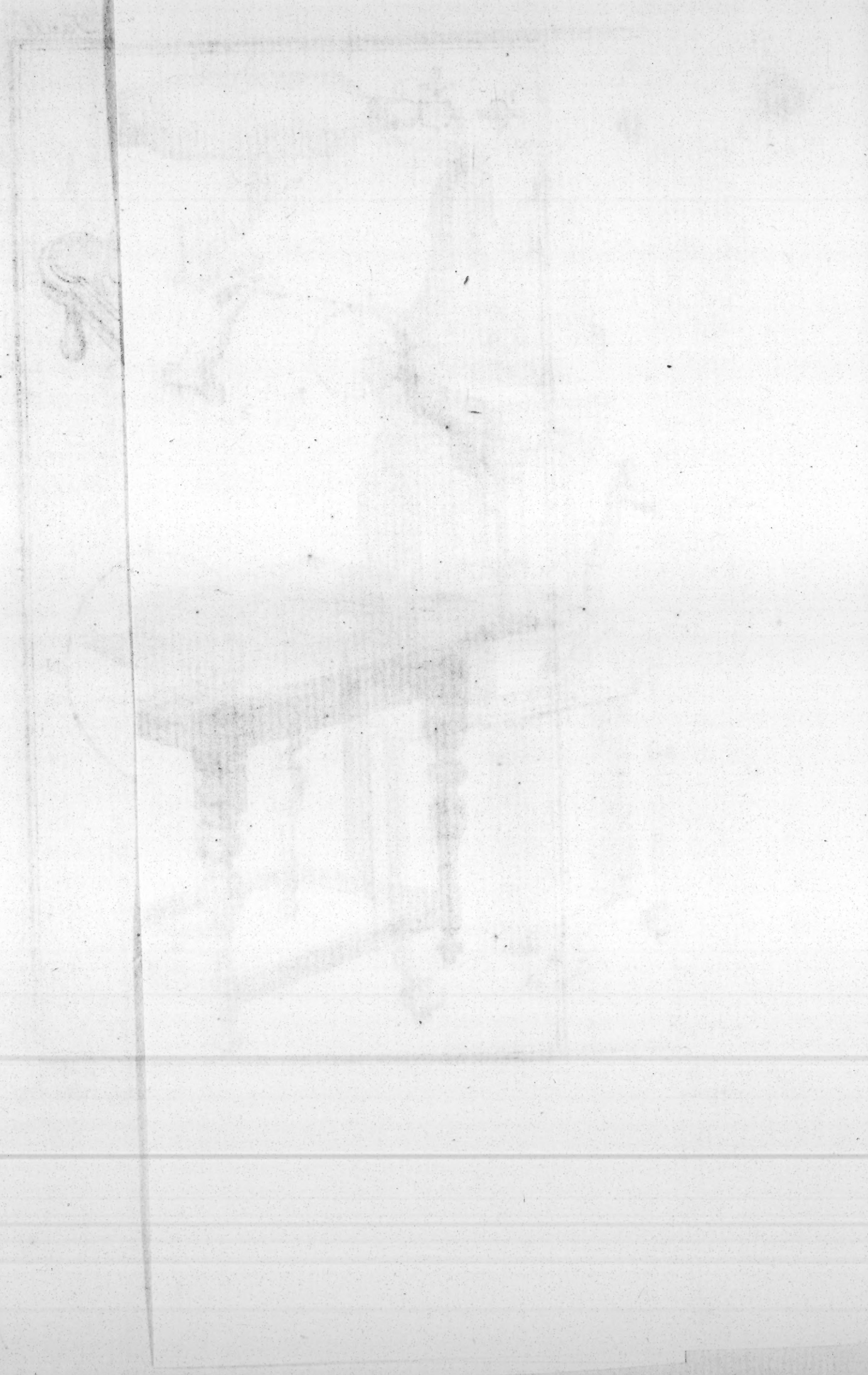
112. **I**N general, this law of indication consists in the difference of the appearances exhibited by the electric fire, when issuing from a blunted point of metal (Pl. I. Fig. 2, 3.) placed in such situations that the electrical fire may, according to the theory, issue from one of the systems and pass into the other. If the point has been annexed to that of the systems from which the fire *issues*, and is properly directed towards a plain portion of the surface of the other, then the fire assumes an appearance that I have distinguished by the appellation of a *brush*; but when the point is annexed to the system into which the fire *enters*, then the fire assumes another appearance, to which I have given the name of *little star*.

113. The *brush* has the appearance of a little conical *fascis* of rays, two thirds of an inch, or an inch, or even more, in length. The blunted part of that lucid cone corresponds to the blunted point of the metallic rod; and there the electrical fire has its greatest density: from such point, the fire subdivides itself into rays which grow continually more numerous, and proportionably thinner, as also more languid. Neither are those rays perfectly continuous; but, if you observe attentively, you will see that they vibrate in an interrupted manner; and you may observe likewise a noise, or rather series of little cracks, separated by little intervals of time, which exactly correspond to the above-mentioned interrupted vibrations; these cracks arise, as do all other sounds, from the motion of the air, and shew by their interruption that the electrical fire strikes the air with a corresponding interruption. This interruption is greater as the metallic rod is blunter; so that if the point of it be extremely obtuse and placed very near the system into which the fire passes, the brush then degenerates to a series of little sparks evidently separated from each other. On the contrary, as the point is sharper, the rays that form the brush grow less divergent, shorter, and more continuous; and the noise degenerates to a whistling, or noise more acute and continuous.

114. The







114. The *little electric star*, as we call it, is very easily discerned from the *brush*: the light of it extends itself to such a short distance that it can hardly be said to form the beginning of a cone, so that many persons give it the appellation of the *lucid point*; this little star produces likewise a little whistling, but all other circumstances being equal, less than that produced by the brush; and in the same manner as the vibration and interruption of the rays of the brush is less manifest at the top of it, where its fire is more dense and affords a more vivid light, so in the short and pretty vivid light of the little star, scarcely any interruption is to be distinguished.

115. I shall treat of the other accidents of those lights in a chapter apart, where I propose besides to enquire into the reason of their formation and effects; our present object being more particularly to define the laws which the position of those lights follow. I. In none of the systems, either a brush, or a little star, ever appears on a point so annexed to it as to turn back again toward the same system. II. There is always either a brush, or a little star, on a point annexed to the one of the systems, and directed to the other. III. If the point annexed to the one system, exhibits a brush directed towards the other, the latter then exhibits a little star, corresponding to the former. IV. Of the three systems, the two which are animated by the friction of the glass, always exhibit the same kind of light, that is, the Chain always a brush, and the Machine always a little star; which lights are more vivid in both, when they take place between one another, than when they arise between them and a strange body: but the third system, that is, the strange bodies, which is not immediately animated by the friction of the glass, exhibits either the brush or the little star, viz. always the little star with respect to the Chain, and always the brush with respect to the Machine. V. The friction of the glass being once supposed, the vividity and duration of the brush from the Chain, relatively to strange bodies, or of the little star on strange bodies, relatively to the Chain, is proportioned to the small, or great, or infinite capacity of the Machine; and *vice versa*, the vividity and duration of the brush on strange bodies, relatively to the Machine, and of the little star on the Machine relatively

relatively to strange bodies, is proportioned to the capacity of the Chain, whether it be small, or great, or infinite. Vithly, and more universally: the brush and little star are subject to the same laws of *unity, existence, non-existence, connection, and proportional distribution*, as the other electric signs.

116. Now, we therefore must lay it down as an acknowledged truth, that the *brush* is formed by the electrical fire that issues, and the *little star* by the same fire, when it enters, since both the said brush and little star, are subject to all the same laws to which the other signs are subject (115, VI. of this the Franklinian theory is an immediate consequence) that is to say, I. All electrical signs are formed by the substance of the electrical fire when it issues from one system to enter the other. (115. III.) II. The electrical signs that take place between the Chain and strange bodies, are produced by the electric fire which issues from the same Chain, and diffuses itself into the ground: the signs between the ground and the Machine are produced by the fire that issues from the ground and enters into the Machine; and the signs between the Chain and the Machine, are formed by the fire, which, with a still greater force, issues from the Chain and enters into the Machine. (115. IV.) III. The fire that rushes from the Chain is proportioned to the quantity which the capacity of the Machine can supply; and the fire which from the ground may enter the Machine, is proportioned to the quantity which the glass can transport from it into the Chain. (115. V.) IV. Hence the Chain is electric by excess with regard to strange bodies; the Machine is electric by deficiency with respect to the same bodies; and the Machine and the Chain are electric per excess and deficiency, with respect to each other.

117. Consequently, the contrary reciprocal effects of the brush and little star, the diversity of their visible form, and the identity of the substance from which they proceed, all these claim this hypothesis, viz. that the brush is an electrical fire that issues, and the little star an electrical fire that enters. The contrariety of their reciprocal effects is manifest from this; if any insulated body whatsoever, be placed in the middle between the said contrary signs when these are similarly effected, or between similar, but

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contrarily disposed signs, the one destroys the electricity which the other communicates. I insulate myself and present my right hand to a point annexed to the Chain, and the left to a point annexed to strange bodies; on the one the brush appears, on the other the little star, and the result is that the one suppresses the electricity raised by the other. The same obtains, if being likewise insulated, I present with my right hand a point to the Chain, and with the left I present another to strange bodies; the brush from the latter will destroy the electricity introduced by the former. The effect will again be the same, if I present one of my hands to a point annexed to the Chain, and a point to strange bodies, a brush from the latter will destroy the electricity introduced by the brush from the former; lastly, the same will obtain still, if I present a point to the Chain, and my hand to a point annexed to strange bodies. Therefore, those signs which, according to the hypothesis, usually manifest such fire as actually proceeds from the insulated body to strange bodies, destroy the electricity introduced by those signs which are used to manifest such fire as actually proceeds from the Chain to the insulated body.

118. Now, if the contrariety in the reciprocal efficiency of the brush and star upon each other, does not proceed from contrary motions taking place in the electrical fire that forms both, we must necessarily have recourse, in order to explain the aforesaid contrariety, to the combination of two different substances, which, when united, reciprocally destroy that electricity which, when separated they are able to raise; but the possibility of such a diversity of substances is excluded by the similiarity of all the perceptible qualities, of the substance that produces the brush, and of the substance which produces the star, and the identity of all the effects produced by both.

119. Indeed, as far as our senses can penetrate into the said brush and star, it evidently appears that both are formed of a substance which shows itself to be equally fluid, equally capable of producing light, equally free of any fumes, exhalation, and makes on all our organs the most identical impressions, except indeed where some difference arises from a difference in the quantity of it; but then it by no means follows, that there is a diversity in the sub-

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stance itself, since it is even possible at all times to transform the star into a spurious brush, and the brush into a spurious star. To effect such a transformation, I use as points, small rods of brass, one eighth of an inch thick, and only rounded at their extremities; when from the one of these a most vivid brush springs out, I present another point in an oblique direction, and it happens that at a certain degree of obliquity and at a certain distance, the rays of the brush, on the former point, considerably lose of their divergency, and turn as if they intended to unite together on the point of the latter; but afterwards they disappear, and are rendered again visible at a certain distance from the said rod, where they again unite together under the shape of a brush.

120. The transformation of the brush is more obvious; we shall see in its proper place, how it is sufficient for making the fire exhibit the appearance of a star, to make it spring from a very sharp point, or to a blunted point to present a sharp one.

121. And these transformations do in no wise destroy the signification of the two signs. For concluding that the electrical fire actually issues from a given system, or enters into it, it is not material whether it always issues or enters with a same appearance, and suffers no change in that respect from any combination that may have been made between the form and position of the body from which it issues, and the form and position of that into which it enters, and from any subversion, as it were, of the said *data* and combination of other circumstances; it is sufficient that the given system at the same time that it throws a brush from a point annexed to it, makes at the same time a little star appear on a point annexed to the other system.

122. Meanwhile, a diligent observator will not fail to observe, especially in the transformation of the star, certain accidents which, though they may of themselves exhibit the appearance of a spurious brush, (119.) yet merely proceed from the rays of the true brush. Because, I. At a certain distance, and with a certain obliquity of the two rods, one part alone of the rays of the true brush, that is, the nearest rays, will deviate and proceed pretty directly towards the other rod, in order there to form the brush; while the farthest will gradually deviate less. II. And the rays
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of the spurious brush will correspond one by one, to those rays alone of the true brush, which will deviate enough to proceed from the rod annexed to the Chain, to that which from the ground is presented obliquely to it. III. And, as the rod that communicates with the ground is brought gradually nearer, other and other rays, which at first deviated less towards it, will gradually deviate more. IV. Such successive deviation of the true brush will still more evidently appear, if instead of the rod, I present obliquely the extremity of my finger; in such case the spurious brush shortens still more, and draws still nearer to the appearance of a star. V. But a still better exhibition of the motion of the true brush in the case of the N^o 115. may be given. Two insulated men communicate, the one with the Chain, the other with the Machine; the former presents the rod, partly to the finger of the latter, partly to the finger of another man who stands on the ground; and then the brush that springs from the rod held by the man communicating with the Chain, exhibits a spectacle equally agreeable and significative; this brush approaches itself with a kind of preference to the man who communicates with the Machine, its deviation towards him begins at a greater distance than usual, is more considerable, and takes place sooner; the diverging rays are more re-united and thrown at a greater distance. All which lays, as it were, the theory under our eyes, or at least is most conform to it, and extremely capable to enable us to distinguish the true brush from the spurious, and to preserve to the former its signification. Mean while, it is easy to perceive in the pretty long rays of the spurious brush, the identity of the substance destined to form the star, with that which forms the brush: which is the object we proposed at first to demonstrate.

123. But such identity is still better confirmed by the identity of the effects produced by the substance which forms the brush, and that which forms the star. Let us suppose for a moment that those two substances are different: now, since all other vivid electric signs are subject to the same laws as the said brush and star, it follows from such supposition the substances that concur in the formation, for instance, of a spark, will be also different; and when, with the knuckle of my finger, I excite a spark from the finger of

the man A, the substance *formatrix* of the brush, and that which produces the star, must therefore both concur in the formation of the spark raised between the man A and me, since such spark is the result of the force that determines those different substances to move towards each other, in order to meet and unite. Therefore, in any point between the fingers at which those substances may be supposed to meet, a reason will fail why they should proceed farther; whence it must follow that the finger of the man A will be pricked for the greatest part by the substance that forms the brush, and my finger by that which forms the star: but how can such most similar punctions be produced by substances of a different nature?

124. Moreover, let the extreme terms of the spark be changed, let its intensity be increased, let even the medium through which it passes be changed; that is, let an efficacious spark be drawn through any such body as usually refuses a passage to it, either in consequence of its own insulating nature, or of its small capacity, then the spark will shiver it to pieces, or reduce it into smoke, or calcine or vitrify it, and in a word, alter it conformably to its peculiar nature, though alter it in the most identical manner, even in those parts which are only applied to each other, and where the substance of the brush alone might then be supposed to operate on the one of those parts, and the substance of the star, on the other.

125. But in order to manifest still more the feebleness, and even the contradiction of the theory of the two distinct substances, I think that I must above all produce the most elaborate and ingenious exposition that has been given of it till this day; such is that which Dr. Priestley has endeavoured to give, and has set in its most advantageous light in his valuable History of Electricity, which he published in London in the year 1767, and which he has enriched with many new experiments and observations of his own.

126. " Let us suppose (says he) that there are two electric fluids,
 " which have a strong chymical affinity with each other, at the
 " same time that the particles of each are as strongly repulsive of
 " one another. Let us suppose these two fluids, in some measure,
 " equally attracted by all bodies, and existing in intimate union
 " in their pores, and while they continue in this union to exhibit

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“ no mark of their existence. Let us suppose that the friction
 “ of any electric produces a separation of these two fluids, causing
 “ (in the usual method of electrifying) the vitreous electricity of
 “ the rubber to be conveyed to the conductor, and the resinous
 “ electricity of the conductor to be conveyed to the rubber. The
 “ rubber will then have a double share of the resinous electricity,
 “ and the conductor a double share of the vitreous; so that, upon
 “ this hypothesis, no substance whatever can have a greater or
 “ less quantity of electric fluid at different times; the quality of
 “ it only can be changed.

127. “ The two electric fluids, being thus separated, will begin
 “ to show their respective powers, and their eagerness to rush
 “ into reunion with one another. With whichever of these
 “ fluids a number of bodies are charged, they will repel one an-
 “ other, they will be attracted by all bodies which have a less
 “ share of that particular fluid with which they are loaded, but
 “ will be much more strongly attracted by bodies which are
 “ wholly destitute of it, and loaded with the other. In this case
 “ they will rush together with great violence.

“ 128. Upon this theory, every electric spark consists of both
 “ the fluids rushing contrary ways, and making a double current.
 “ When, for instance, I present my finger to a conductor loaded
 “ with vitreous electricity, I discharge it of part of the vitreous,
 “ and return as much of the resinous, which is supplied to my
 “ body from the earth. Thus both the bodies are unelectrified,
 “ the balance of the two powers being perfectly restored.

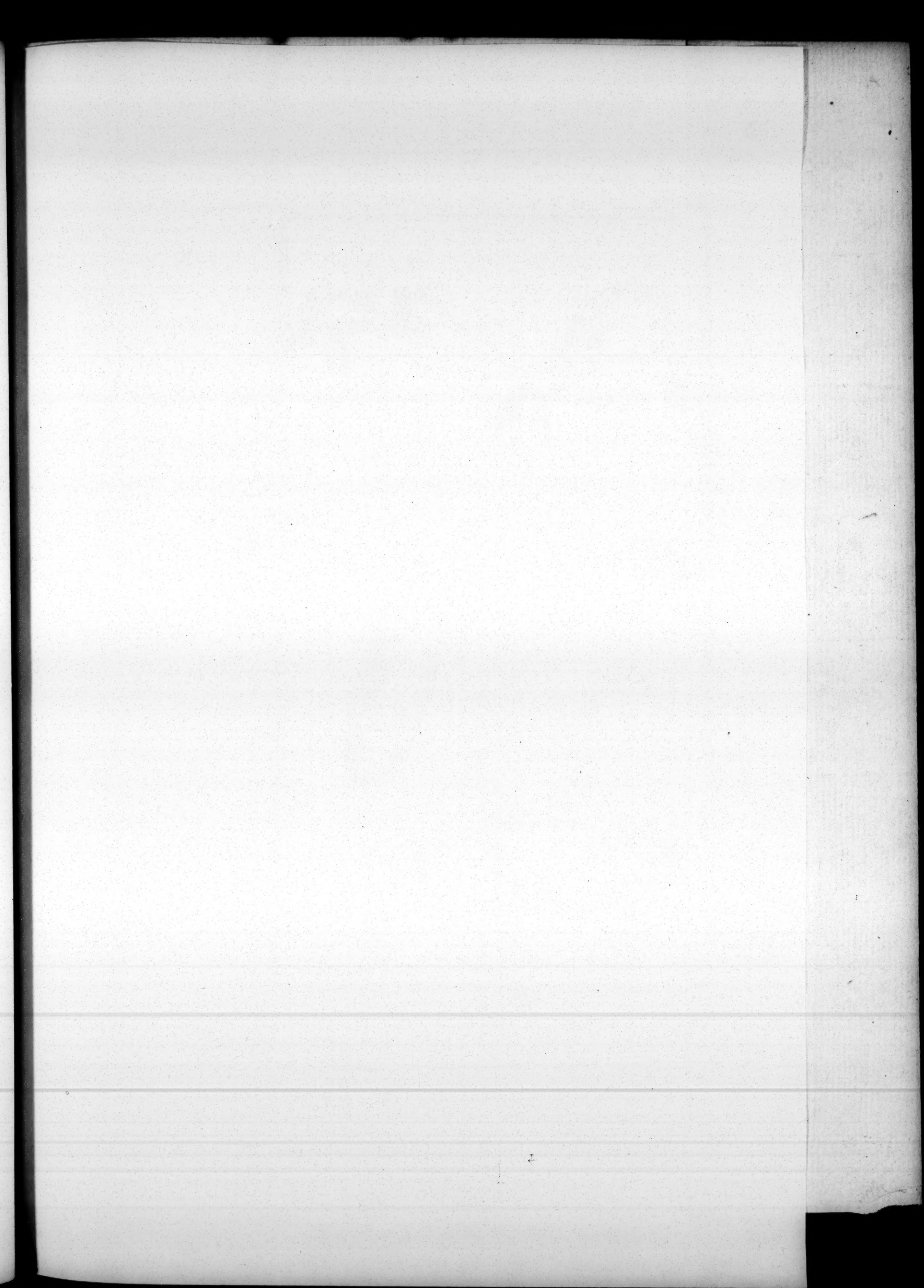
“ 129. When I present the Leyden phial to be charged, and,
 “ consequently, connect the coating of one of its sides with the
 “ rubber, and that of the other with the conductor, the vitreous
 “ electricity of that side which is connected with the conductor
 “ is transmitted to that which is connected with the rubber, which
 “ returns an equal quantity of its resinous electricity; so that all
 “ the vitreous electricity is conveyed to one of the sides, and all
 “ the resinous to the other. These two fluids, being thus sepa-
 “ rated, attract one another very strongly through the thin sub-
 “ stance of the intervening glass, and rush together with great
 “ violence, whenever an opportunity is presented, by means of

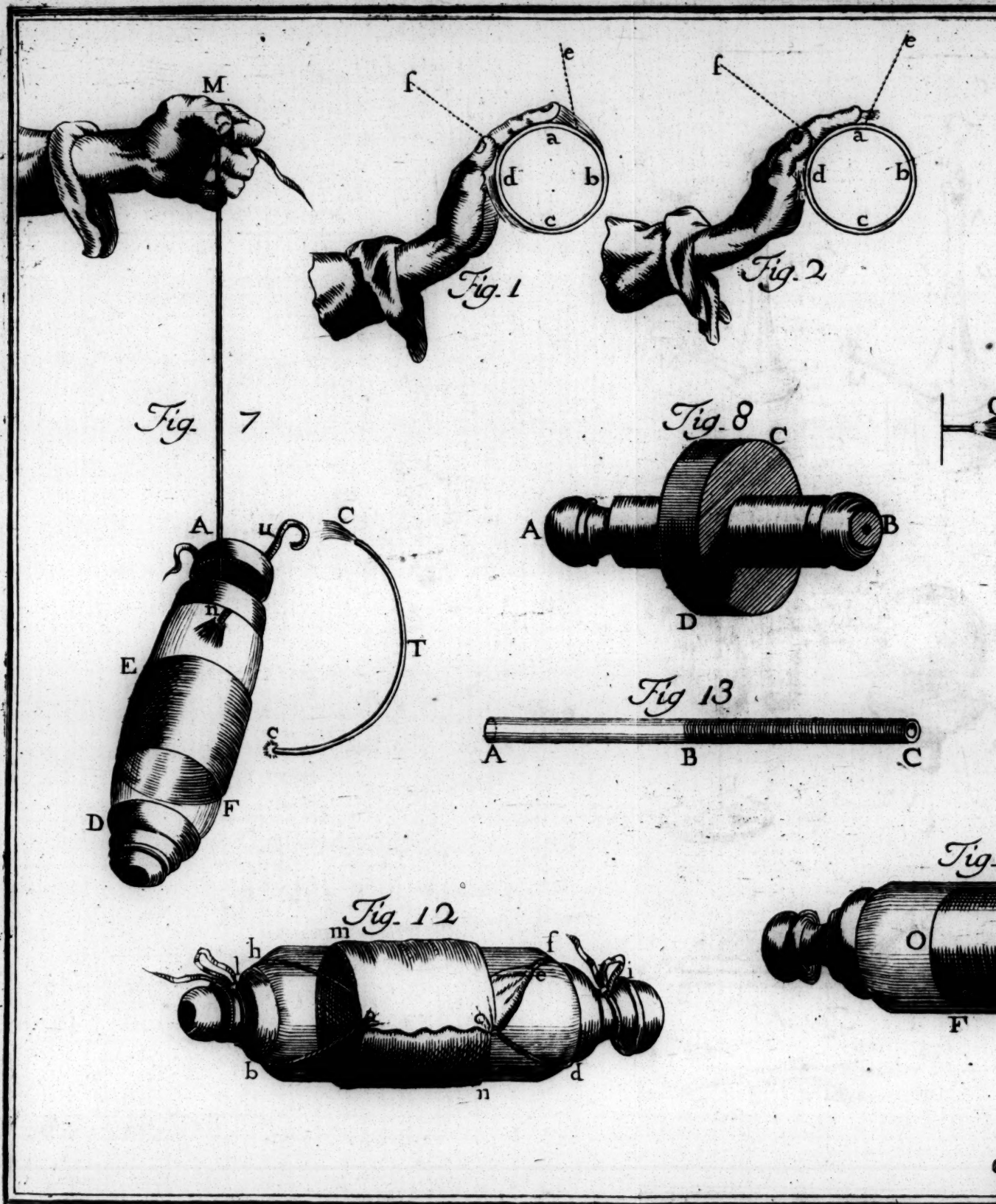
“ proper conductors. Sometimes they will force a passage through
 “ the substance of the glass itself; and, in the mean time, their
 “ mutual attraction is stronger than any force that can be applied
 “ to draw away either of the fluids separately.”

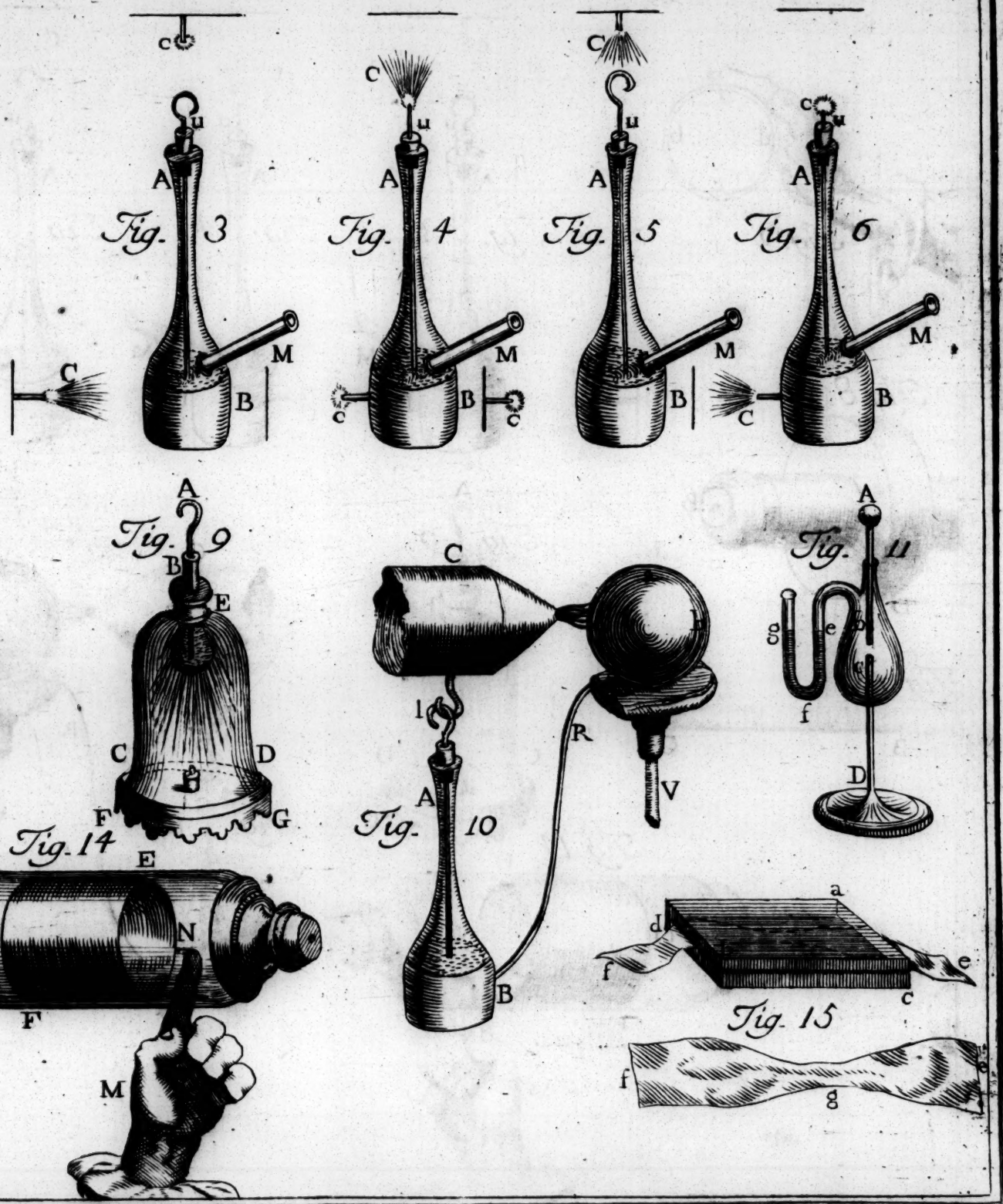
Such are the principles on which Dr. Priestley establishes the hypothesis of the two fluids; and I think I must here observe that the existence of two fluids being once supposed as fact, the principles above appear to me necessary, or at least very proper to explain a few particular facts, or at least a few which can admit more easily of such explanation, and Dr. Priestley, it must be confessed, has set forth and supported the said hypothesis as ingeniously as well could be; but on the other hand I must add (did I omit it, it would be doing injury to the said ingenious author) that in his posterior experiments and observations, he has usually followed the Franklinian theory.

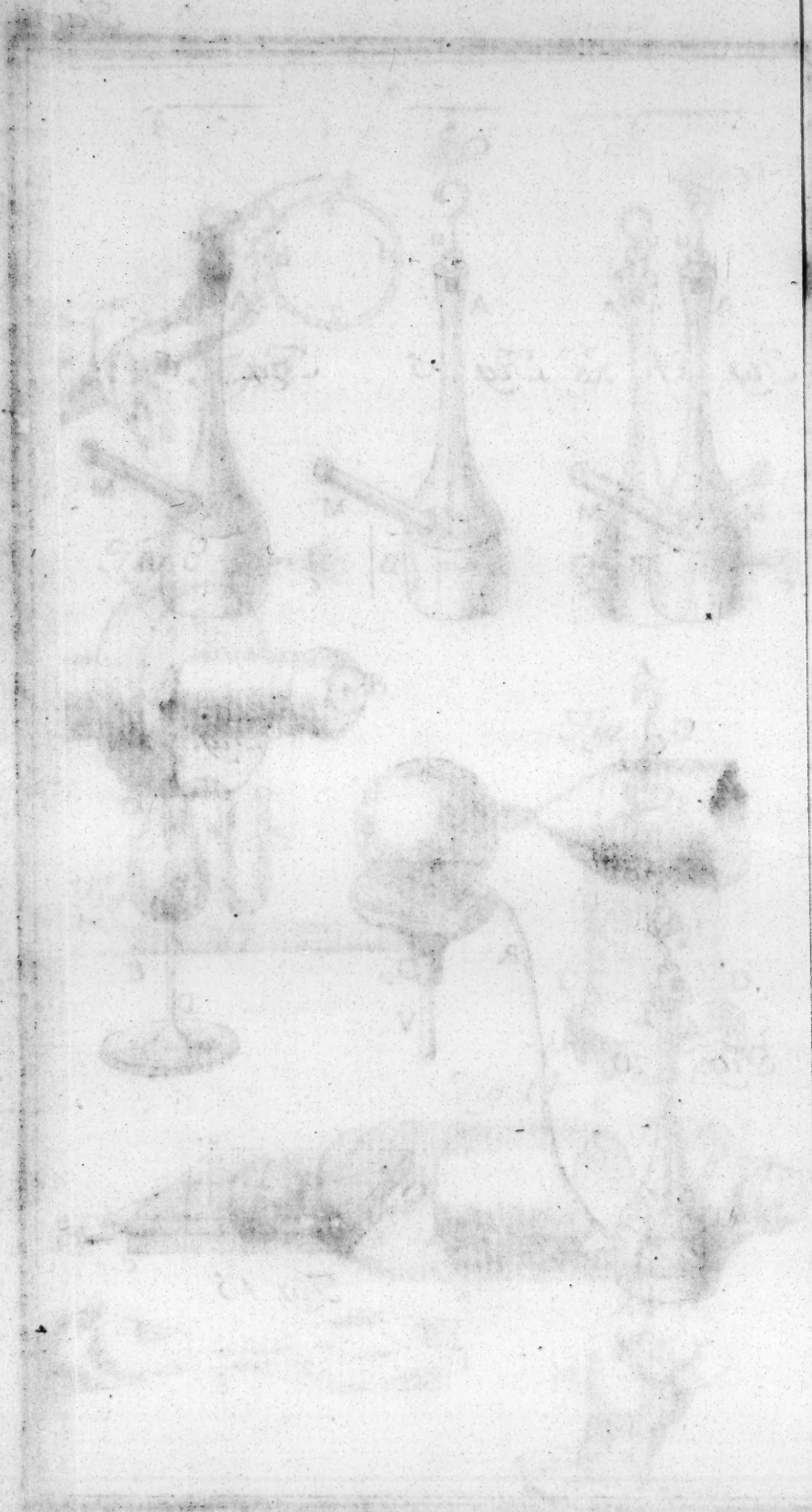
130. After those previous observations, I shall make some reflections on the abovementioned principles, and shall dissemble none of the considerations with which Dr. Priestley has endeavoured to confirm them. And first, with regard to the chymical affinity of two fluids, it is true, as Dr. Priestley alledges in line 27. page 470. that nature abounds with instances of affinities of this kind, which, when the substances are united, do not manifest their peculiar force, though this force is exerted in such a remarkable manner when these substances happen to be separated: but indeed I have no knowledge that there can be in nature any instance of two distinct, nay contrary substances, such as must be the vitreous and the resinous fluid, which, when separated, may have all their outward manifestations exactly alike, and produce the very same effects, the one as the other; besides that such an affinity, however convenient it may appear for the explanation of the natural equilibrium of the two fluids, is entirely repugnant to the inequilibrium, or *unbalancement* of one.

131. Besides, how is it possible for the friction ever to surmount the union resulting from such a chymic affinity? The friction uniformly excites the contrary kinds of electricity in the several parts however distant, of the Machine or Chain, which in the Franklinian theory naturally and necessarily follows from the mere expansive force of one same fluid. It is enough that the friction should









increase in any manner whatever, the expansive force of that portion of electric fluid which is on the surface of the hand, or of the rubbing cushion, or that the same friction should diminish the expansive force of that portion of electric fluid which is on the surface of the glass, for producing a translation of a portion of that fluid from the hand to the glass; and the same circumstances likewise suffice for making the fluid in any part, however remote, of the Machine, to distend itself proportionably to the quantity wanting in the place of the friction, and causing the *excessive* fire carried by the revolving glass to the Chain, to pass into the same, and create in all parts of it an *excessive* density. But in the theory of two fluids, how the action, entirely mechanical, of the friction (which does not of itself extend farther than those parts which are actually brought into a contact with each other) can it possibly separate in very distant places and by a positive force, different parts of two fluids which are closely united with one another? A particular principle, or force, should at least exist, which, after the separation of those parts of the two fluids which are near the two rubbing surfaces has been effected, could determine the other parts diffused throughout the whole extent of the Chain or the Machine, to pass to one same place, in order there to be in their turn disunited and separated. Now, the hypothesis of two fluids does not mention any such active principle; on the contrary, it supposes a force that keeps all the parts of the fluids united, and the excogitator of it moreover confesses (p. 475. l. 11.) that each of the fluids is attracted by all bodies around it, with a force at least equal to that with which they reciprocally attract each other.

132. I here omit to mention that the electricity in the Machine, and the Chain, are animated in a very different way: in the Machine it is animated by a friction of a part of the same against the glass; in the Chain, by a mere passive vicinity of the same to the glass. This, in the Franklinian hypothesis, is entirely consistent: it suffices that the glass, through the mechanical force of the friction, takes a quantity of electric fire from the Machine, to make the same diffuse itself, in consequence of its natural expansive force, into the Chain. But in the hypothesis of two fluids, it becomes necessary that
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the friction, which only obtains between the glass and a very small part of the Machine, should equally separate the resinous from the vitreous fluid, both in the Chain and the Machine; and the former must as well be transported from the Chain into the Machine, as the latter from the Machine into the Chain.

133. But Dr. Priestley is indeed far from being persuaded that the friction (which, we must observe, is acknowledged in both hypotheses to be the only cause of the contrary electricities in the Machine and the Chain) can without a contradiction separate the two fluids, and reciprocally transport the resinous fluid of the Chain into the Machine, and the vitreous fluid of the Machine into the Chain: on the contrary, he really seems to have perceived that things were otherwise, and not to have found any other better way of favouring his hypothesis, but by substituting the unsupported words *let us suppose*, to argument and facts.

134. The disjoining of the two fluids being once gratuitously supposed, the same gentleman passes to the application of his hypothesis to electrical motions; to which motions the said hypothesis appears to him extremely conform, while he thinks the Franklinian theory insufficient: *indeed Dr. Franklin himself*, says Dr. Priestley, in p. 472. *ingenuously acknowledges that he was a long time puzzled to account for bodies that were negatively electrified repelling one another.* The same difficulty likewise occurred to me at first; but it had not with me, and I think with great reason, the same weight it must have had with others, because, I. Even then I did not consider the divergences as being produced either by the excess, or the defect of the electric fire, absolutely considered; but indeed from the inequality between the fire in the bodies, and that in the ambient air (Chap. II.); and as this inequality subsists in the cases of bodies electric by excess, as well as in the cases of bodies electric by deficiency, the divergence in both cases appeared to me equally natural. II. Afterwards, in the year 1760, the consideration of the electricity of the vaporous air (the experiments relative to which I had exposed in the seventh letter on the terrestrial atmospheric electricity) induced me to write a letter to Dr. Franklin, published in the second part of the 50th volume of the Philosophical Transactions,
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in which I explained more precisely the divergence of bodies equally electric, by the sole pression, either of the fire superabounding in bodies against the fire in the air; or the pression of the fire in the air, against the fire in such bodies as were deficient. *Si ignis proprius corporum, quantuscumque is sit* (I say in p. 522 of the said volume) *libratur cum igne ambientis aëris, corpora . . . consistunt in nativo statu; . . . si alter ignis superat alterum, corpora discedunt, vi ignis superantis.* 3dly, and lastly, Since the time I succeeded in analysing the phenomena of the electric well, and thus to ascertain the nature of electric atmospheres, I have continually found the explanation above more true and consentaneous to all manner of facts. I here desire the reader to consult my little book *de Atmosphæra Electricâ, ad Regiam Londinensem Societ. Taur. 26 Feb. 1769.* The fire inherent to the air tends to balance itself, but without mixing with the moveable fire in the deferent bodies; and in the same manner as a same kind of motion is perceived in the balance, whether I put a new weight in the side A, or take an equal weight from the side B, thus two bodies equally electric always will equally diverge, whether the excessive fire of the bodies surpasses the natural fire in the air; or the natural fire in the air, surpasses the deficient fire in the bodies.

135. But we are to discuss all this in time, with more precision and accuracy; now it suffices, after having shewn that divergences are no wise repugnant to the Franklinian theory of a single fluid, to observe, that though these divergences may at first sight seem to favour more the theory of the two fluids, yet, in examining more closely into the subject they are found to render that theory much less probable. I omit, for the present, the difficulty of separating the two fluids mentioned above; I only consider here the impossibility of conciliating the identity of the effect produced by each of those supposed fluids severally, in the body in which it is supposed to predominate, with the supposed difference of their nature, in consequence of which, it is said, they mutually attract themselves. This is the character of a philosophy illusive, and too lazy to inquire into the causes of things, thus to imagine fluids that must have such motions as cannot take place in bodies. Bodies differently electrified mutually attract each other: Why?

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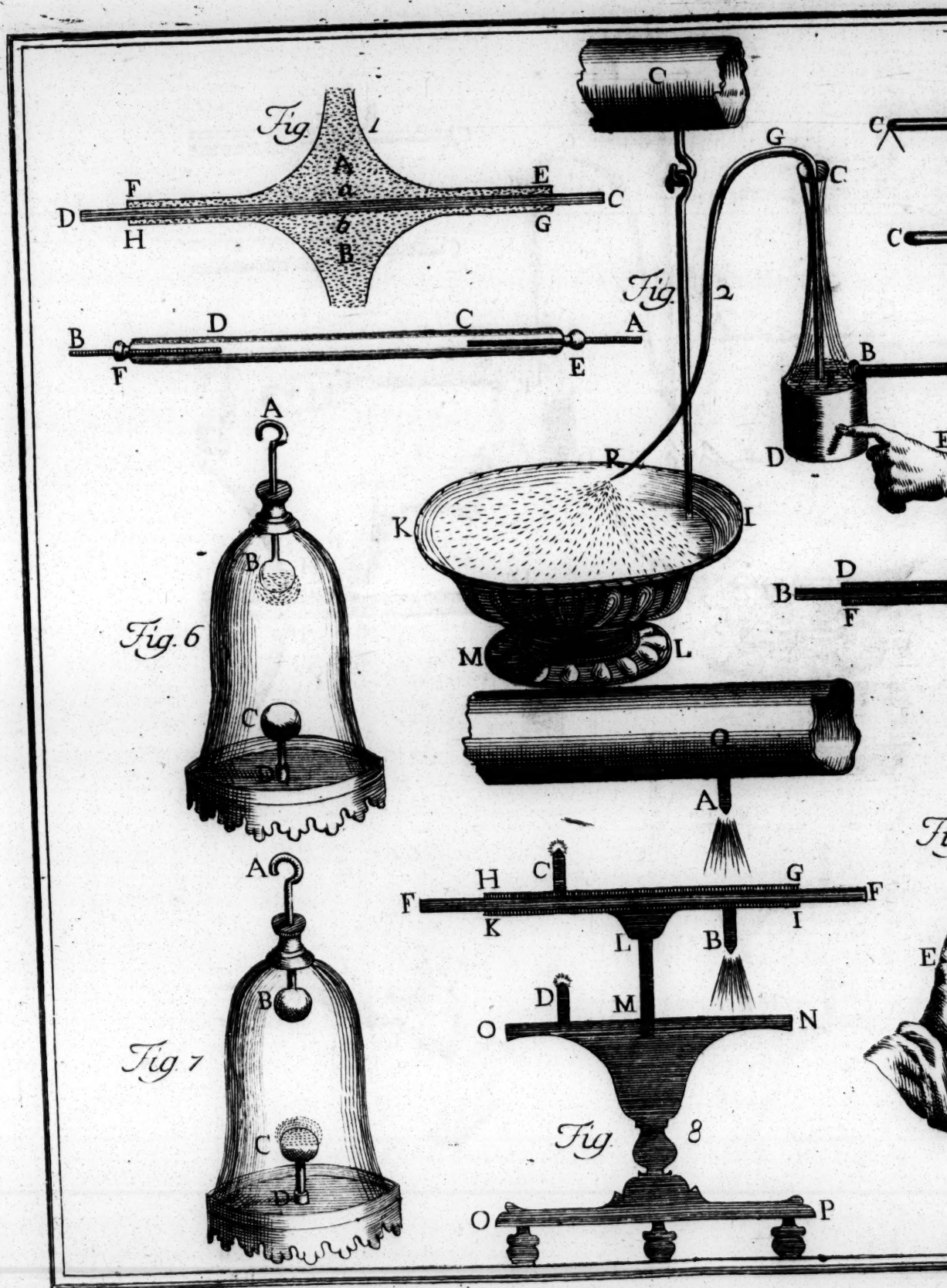
Because

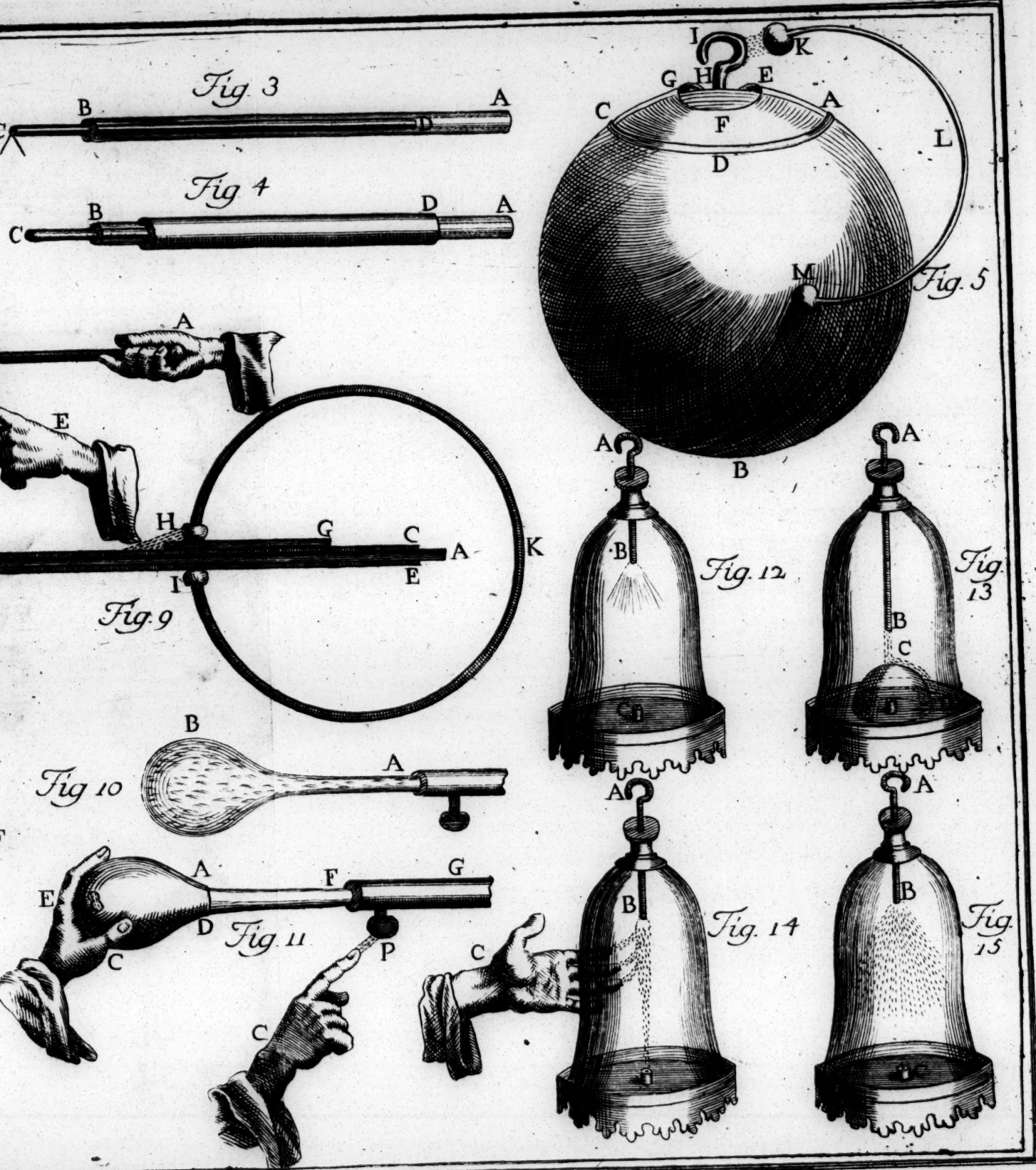
Because they are animated by two fluids that mutually attract each other. Bodies similarly electrified mutually repel each other: Why? Because they are animated by two fluids, the particles of each of which repel each other, and that in a most similar manner. But that these are by no means the principles and ways of nature, is manifest from the exact unity of the effects of those supposed fluids, which unity is absolutely repugnant to the supposed diversity of their natures; from the impossibility of their separation, on which separation, nevertheless, every one of their manifestations must depend; lastly, from the manner after which they should unite again; which would be intirely opposed to all phenomena.

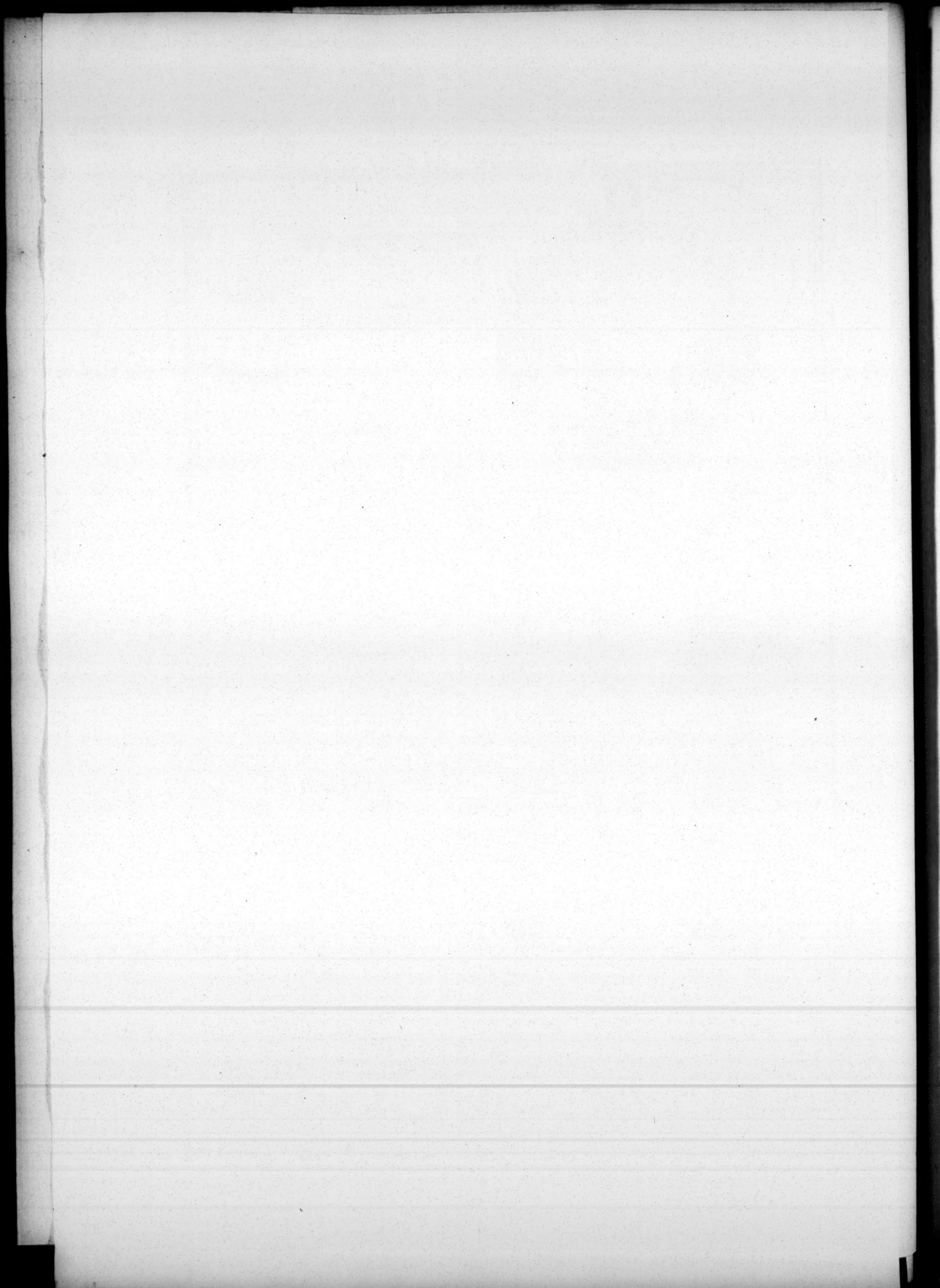
136. With regard to the two last circumstances, " If it be asked, " (to use the words of Dr. Priestley, p. 475.) why the two fluids, " meeting on the surface of the globe, or in the electric explosion, do not unite, by means of their strong affinity, and make " no further progress; it may be answered, that the attraction " between all other bodies and the particles of both these fluids " may be supposed to be, at least, as strong as the affinity between " the fluids themselves; so that if any body is dispossessed of one, " it may recruit itself, to its usual point of saturation, from the " other *." But, indeed, to attempt, in an hypothesis contrived as this is, to resolve one difficulty, only serves to raise a number of others. First, if the fluids are more strongly attracted by the bodies in which they are contained, than they attract each other, certainly the difficulty of separating them must become double; since we must, besides their affinity and mutual attraction, also surmount this new common attraction that keeps them united to the said bodies: to this add, that such attraction must be surmounted only with regard to the fluid that is to be separated: besides, would not such an attraction from the bodies oppose the repulsive force of the separated fluids? Secondly †, how could those impulsions of any one of the

* Let us observe, that in the surface of the glass, at least in that portion of it that proceeds from the hand towards the Chain, the electricity by excess actually predominates; which would be impossible in the hypothesis of the two substances.

† I suppose still that these words *let us suppose* have the miraculous efficacy to cause the two fluids, viz. the resinous from all sides of a Chain ever so extensive, and the vitreous from all sides of a Machine, let its capacity be ever so great, to run to the glass.







fluids towards the Machine and the Chain, instead of balancing each other, surmount the mutual attractions of the fluids towards each other, and their common attraction towards the glass; and operate so as to make those fluids continue to exchange their stations and proceed, viz. the vitreous into the Chain, and the resinous into the Machine. The same reasoning must be applied to the case of two fluids which are supposed to encounter each other in the line in which an explosion is effected: there is no doubt but such explosion can be effected through a deferent body, for instance, through an iron wire several miles long. Now, why should those fluids, which are supposed to meet in the middle of the said line, instead of being stopped there by the efficiency to their affinity, and of the attraction of the iron within which they are supposed to run, why should they, I say, preserve their motion and proceed farther? This surely must be in consequence of some attraction from the surface of the glass; but then, it is requisite that such attraction should be infinite (since it must be subject, like all kinds of attractions in nature, to some inverse law of distances) with regard to the sum of the abovementioned forces.

137. I omit, as being absolutely superfluous, to mention the other arguments by the help of which Dr. Priestley continues, in page 475, to endeavour to procure some probability to the hypothesis of two fluids; which indeed of itself can have none: and whoever will consider it with any degree of attention will be made still more sensible of its contradictions than I have been able here to manifest. The able historian of electricity has himself given the best confutation of his own theory of two electric substances, the resinous and the vitreous, by attempting to apply it to general facts, though, it must be confessed, he has in the mean time used all possible industry to reconcile his theory to those facts.

138. I think I cannot better confirm the theory of a single fluid, than by producing here a most significative experiment, which I sent the 14th of January, 1766, to the Royal Society, and which is inserted in the volume of the same year. Let two balls of brass exactly polished B C, two thirds of an inch, or an inch in diameter, be placed under a bell of glass (Pl. IV. fig. 6, 7.) at

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the distance of about five inches from each other. The ball C rises from the basin of the air-pump; the other B is annexed to the rod A B which communicates, through the neck of the bell, with the Chain. I place the bell upon the air-pump (Pl. II. Fig. 9.) and, when the air is carefully extracted, I rub the glass softly, in order to excite in the Chain only a very weak electricity; meanwhile I observe, and explore the precise degree to which the electrical fire manifests itself on the ball B, without passing, at least visibly, to the ball C: usually I obtain such precise degree, by applying the palm of my hand to the side of the bell, taking care not to touch the brass with which the neck is garnished: the consequence of which is, that as a great part of the bell is externally coated, a great portion of fire flows along that part of the interior surface which corresponds to my hand, and it at last happens that that precise portion becomes retarded, which is necessary for producing the desired appearance.

139. This appearance consists of a most agreeable and regularly variegated hemisphere of light, which rises from the lowest hemisphere of the ball B in *b*, and covers the same, while, at the same time, no other light is perceived, either on the other ball C which communicates with the ground, or in the space between both. The disposition and degradation of that hemisphere consists of a series of circles having for centre the lowest point of the ball, and growing gradually larger as they rise around the inferior hemisphere of it. I. The light which produces the rings formed between the successive circles, is equally rare, and equally languid in all. II. That height, besides, to which the light reaches, is greatest in the common centre of all the rings, and from thence goes regularly decreasing, according to the greater distance at which the successive rings stand from that centre. III. The rarity, and languidity of the light, increase after a same manner.

140. Now the manner and order, after which that light, which is of one and the same kind, rises from the ball that communicates with the Chain, does certainly suffice to render visible to the eye, I think I may so express myself, the theory of a single fluid, and to confirm all that has been said before, to prove that the brush is produced by such a single fluid issuing from a system that is in a super-

superabounding state relatively to another. But the preceding experiment will receive a new degree of significancy from the following one, which is similar to it. I make the end of the rod A B which communicated before with the Chain, communicate now with the Machine; and the same agreeable and regularly variegated hemisphere of light now covers the upper hemisphere of the ball C, which has been made to communicate with the basin of the air-pump, that is, with the ground: in the mean time, no light appears any where else, neither around the other ball B, which now communicates with the Machine, nor in the space between the two balls. If I intend now to have the light appear around the lower surface of the ball B, the place where it appeared when the rod A B (Pl. IV. Fig. 6.) communicated with the ground, I insulate the air-pump and make the basin of it communicate with the Machine; I then make the end A of the rod A B communicate with the ground; and then the light appears again in B.

141. In short, those experiments evidently prove that the electrical matter manifests itself on that ball only in which the electrical fire finds itself in a state of a relative excess: that is, on that ball which communicates with the Chain, and in which the fire is excessive, while, in the mean time, the other ball communicates with the ground in which the fire is in the natural state; and on the other hand, the fire manifests itself on the ball that communicates with the ground, when the other ball happens to communicate with the Machine, in which the fire is deficient with regard to the ground: in short, the light appears on the ball annexed to that system which would, through a point, throw the brush toward the other. Therefore it is manifest, that such a light (could not we call it the light of *diffusion*) whether we consider its disposition and form, or examine either its situation or unity, allows no possibility to the theory of two distinct electrical fluids.

142. But the comprehensive signification of this light of *diffusion* is found, on the other hand, to increase beyond measure, when we attend to another light that I call, and which is in fact, the light of *overflow*. This light appears likewise on one of two balls B C placed near one another, under a bell emptied of air: it is a

matter of indifference whether they be placed either the one under the other, or both in an horizontal plane. The vacuum being formed, and the hook of the rod being joined with the Chain, we may then observe a cylinder of a weak reddish colour, flowing from the ball B which communicates with the Chain, toward the ball C which communicates with the ground, and forming there a small light of *overflow*, such as is delineated in Pl. V. Fig. 10. When the electricity is weak and the ball C exactly communicates with the ground, this light becomes like a most subtle fur, or veil, which spreads itself around the basis of the small cylinder of light, in the place where this reaches the ball C, and even such light does not appear but very imperfectly, when the place in which the experiment is made is not perfectly dark.

143. But then that light of overflow will become completely visible if a bottle be discharged between the two balls; a little discharge will produce a small light, such as it is represented in Pl. V. Fig. 10. A greater discharge will present a light proportionably greater, as in Fig. 11. And a still more copious discharge will make the light of *overflow* plentifully surround both the whole ball C, and a part of the rod on which it is raised, and even spread itself around the ball B (Fig. 12.)

144. I have hitherto supposed that the bottle is discharged by directing its excessive fire from the ball B to the ball C; now I direct it in a contrary way. In the last experiment I held the basin of the air-pump with one hand, and carried with the other the hook of the bottle, to the rod of the ball B; now, I carry the hook of the bottle to the basin of the pump, and the result of this change is, that the light of overflow then appears around the ball B.

145. This light, therefore, is of itself, a farther confutation of the theory of two fluids, and an ocular demonstration of the Franklinian theory of one fluid, which diffuses itself every where to an equality; for either that light is formed by one of the two supposed fluids, or by both at once, while they move from the one ball to the other: but if the light in question is formed by both, why does it rise on one ball alone, and not upon the other? If it

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is formed by one fluid only, why does it not rise on that ball from which such fluid moves?

146. But the extensive significancy of those two lights, that is, that of *diffusion*, and that of *overflow*, increases still, if we compare their several accidents together, and examine the agreement between them and the theory of a single fluid. The light of diffusion manifests itself to a great distance, from one ball to the other; the light of overflow loves to keep to small distance: the light of diffusion appears, though neither the luminous cylinder or any other emission of light takes place; the light of overflow does not appear but consequently to the existence of a cylinder of light; that is to say, in consequence of an actual transfusion of the electric fire: the light of diffusion cannot receive any addition without degenerating to one continued ray; the light of overflow increases as the force of the dislodged fire increases. Lastly, the former light appears on the ball from which the electric fire, according to the theory of one fluid, must issue; the latter, on the ball into which, according to the same theory, it is to enter: the former, on that part of the ball superabounding with fire, which faces the deficient ball; the latter, on that part of the deficient ball which faces the ball that superabounds with fire. The nature of the former light agrees extremely with the idea of a fire that issues with an inconsiderable degree of force, and which is not able to repel the residual air, however rare it may be and spread itself to any considerable distance; the latter, agrees entirely with the idea of a fire that enters, and which, striking against the fire originally existing in the ball, recoils back against the moveable rare residual air: in short, that appears in the same place that the brush does, this in the same place that the star does. Therefore, it appears that the signification of the brush and the star is fully demonstrated, as well as the Franklinian theory, which results from the said signification, or rather is contained in it: which was the principal object of this chapter.

C H A P. VI.

On the law of *excitation*.

147. **H**ITHERTO, I have only spoken of the two animated systems, the Machine, and the Chain; and of the indifferent system, or strange bodies. The appearances exhibited by the brushes and the stars, excited between those three systems are most distinct, and the other signs between them are also more decisive, than those on the *animating* systems. However, in order to throw a complete light on the subject, I shall now consider the latter system, that is, the glass; and discuss that law which I call of *excitation*, in consequence of which the electric fire passes from the rubbing hand into the rubbed glass: a part of this fire afterwards flows back into the hand, at that place where the glass begins to move from it; another portion diffuses itself into the Chain, through that part to which the glass passes nearest; and another portion gets back into the hand, at that part where the glass returns again to it.

148. Let us propose that law with more precision. *I. The electric fire, proper to the rubbing hand, passes invisibly from those points of it which are in contact with the glass to the surface of the glass, and adheres to it proportionably to the force of the friction, and to the quantity of fire existing in the hand itself. II. That fire which is thus collected upon and adheres to the glass in the time and in the place of the friction, from that instant begins to diffuse itself from those parts of it which, in consequence of the rotation, begin to leave the contact of the hand; a part of the same fire thence flows back and gets again into those points of the hand which lie nearest to the glass, though no more in contact with it; while the remaining part of the fire keeps on that part of the glass which leaves the hand and goes to the Chain. III. Of that residual part on the surface of the glass a portion gets into the Chain, where the glass passes nearest; which portion increases till it has attained a same ratio to the capacity of the Chain, as the portion which remains has to the capacity of the glass.*

149. That the electric fire passes from the hand to the glass at the place where the friction is effected, we shall find manifest when
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we consider that this glass is the sole moving principle of the contrary electricities in the apparatus; that such electricities are excited with a greater vivacity, as the friction is performed in a better manner and with a properer degree of force; and, lastly, when we see that those parts of the hand which actually performs the friction, are found to be electric by deficiency; and that those parts of the glass which have just left the hand, are found to be electric by excess. Let the circle *a b c d* * design the equator of a cylinder rubbed by the hand B: now, whenever a point is presented to that equator, a *star* appears on that point; but whenever the same point is presented to the hand (the Machine being insulated) a *brush* appears upon it. Nor are we to wonder that in the common way of making experiments † no light appears on those parts of the glass where the friction is effected; this is because the electric fire, in diffusing itself from the hand into the glass immediately contiguous to it, meets with no resistance; now, a resistance is the only cause which ever makes the electric fire become visible.

150. Lastly, that the fire which from the rubbing body passes into the glass, is proportioned to the quantity which is able to be actually supplied from elsewhere to the said body, is manifest from all that has been said concerning the laws of *existence*, and of *connection*; and this I most satisfactorily shew, by rubbing the cylinder or globe with the cushion represented in Pl. I. Fig. 8. which is insulated with a stick of glass: in this case the cylinder or globe scarcely exhibit any electricity.

151. With regard to the other parts of the law, objects of this chapter, they are rendered manifest by the means of the three following lights. I. By the means of a series of little stars which sparkle in *e* (Pl. III. Fig. 2.) on the inferior extremity of the rubbing fingers, that is, where the glass, which is supposed to move according to the letters *a b c d*, begins to part from the hand; those stars manifest that portion of fire which, in that place, gets back into the hand; and I call such light, the light of *separation*. II. By the means of the star that appears on the point of the

* Pl. I. Fig. 1. 6. Pl. II. Fig. 2.

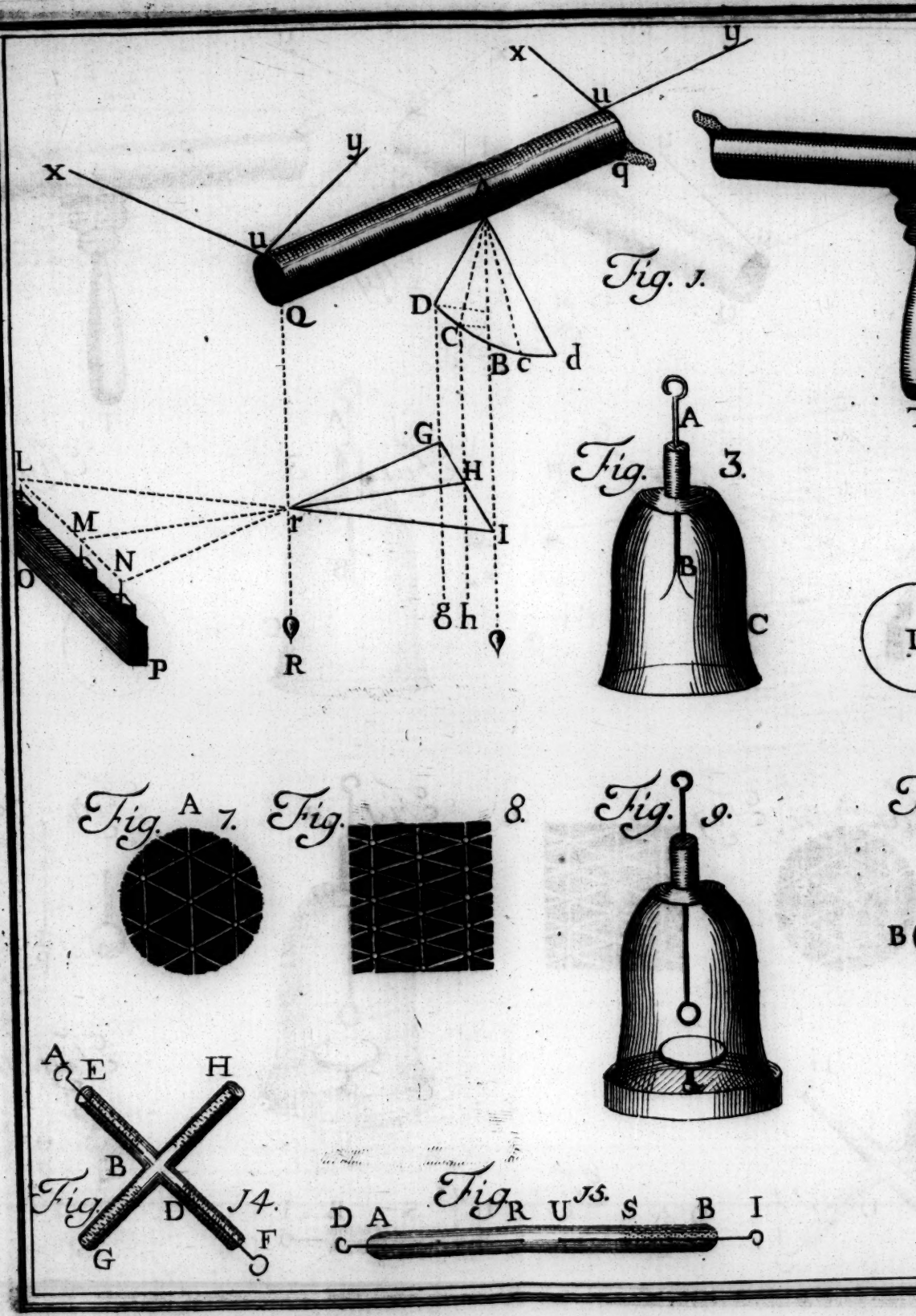
† We shall see afterwards the reason why there appears a light, when the cylinder, or globe, are emptied of air.

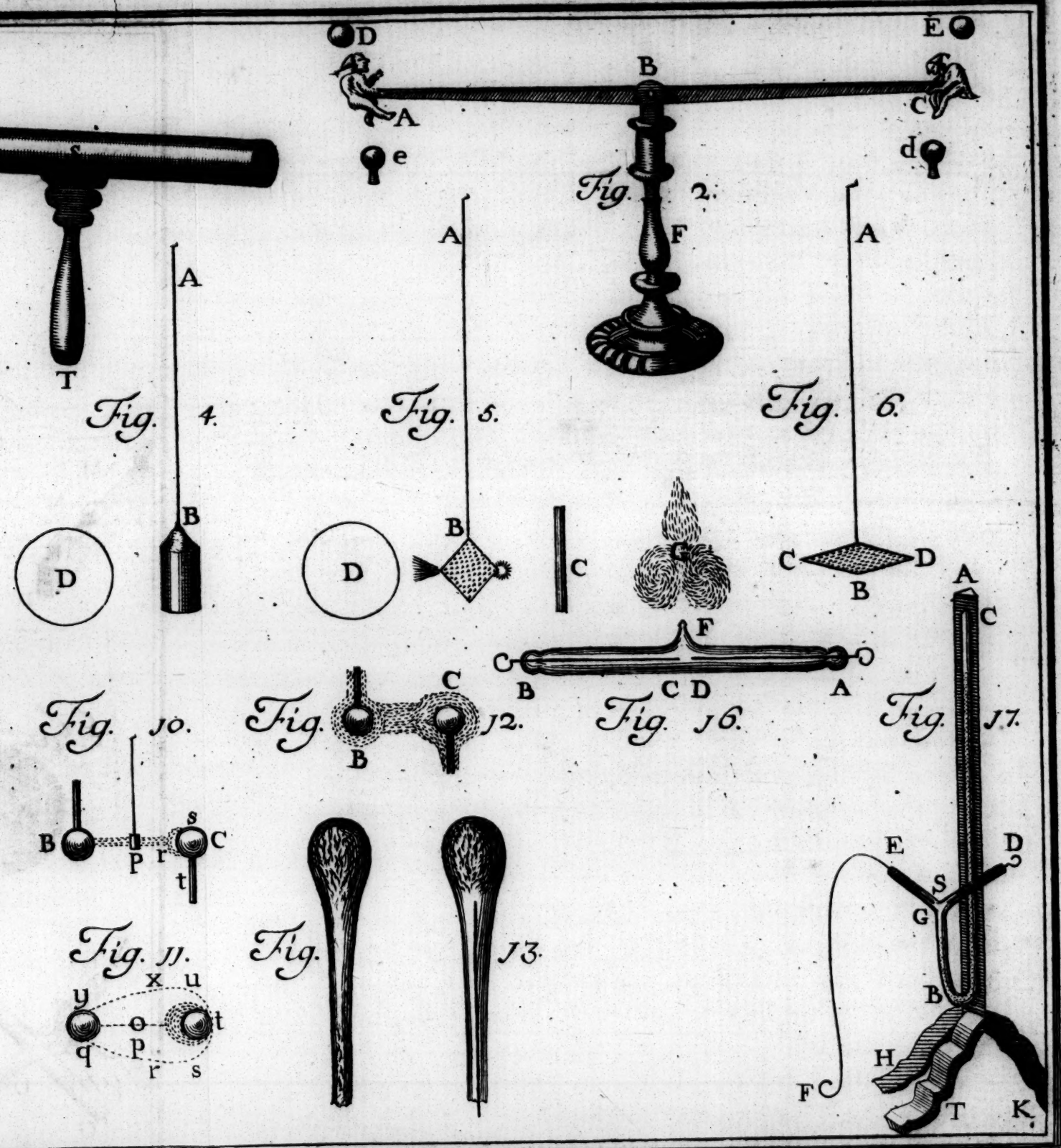
Chain (Pl. I. Fig. 7.) which star I call *light of distribution*, as that light indicates that portion of the electric fire which from the glass diffuses itself into the Chain, proportionably to the capacity of it. III. and lastly, By the means of another series of stars, which appear likewise on the hand in *f* (Pl. III. Fig. 2.) where the glass in revolving returns to the hand: these stars, which I call the light of *return*, indicate that portion of fire which remains on the glass, after it has passed near the Chain.

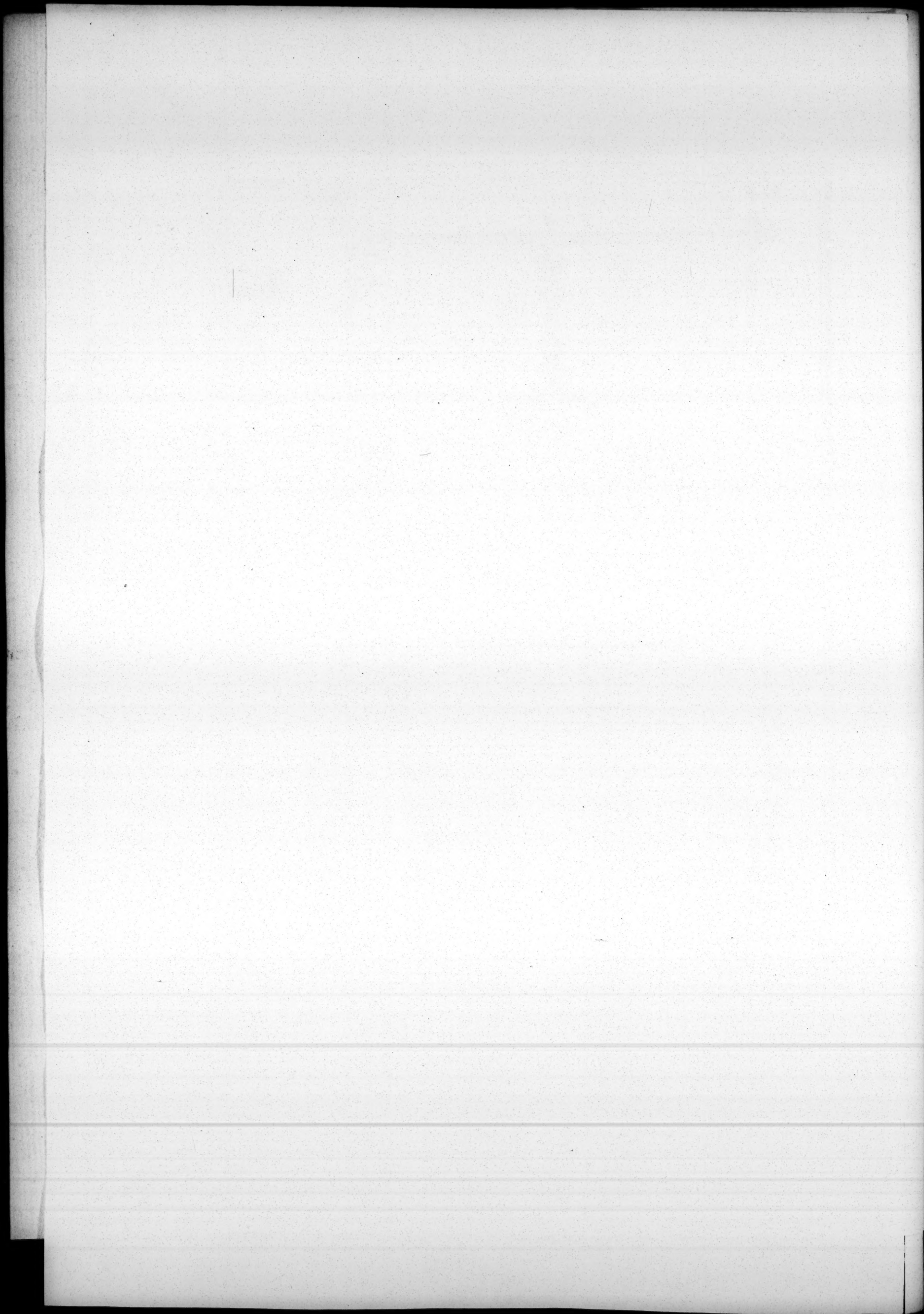
152. That those three lights must be considered as true stars, is rendered manifest by the following transformation I make of them. Instead of a cylinder of glass, I now rub a solid cylinder of sealing wax, and the result is, that instead of seeing on the fingers in *e*, and the hand in *f*, short flattened lights, we now see long tracts of light take place from the fingers in *e* and from the upper part of the hand in *f*: those in *e* follow the wax as it moves away; those in *f* throw themselves out to meet the wax as it returns to the hand: likewise, instead of the star which before appeared on the Chain, there now springs from it a most conspicuous brush. All this is to be explained thus: the wax stick (this property of wax we shall prove more fully afterwards) gives to the rubbing hand a part of its fire, whereas the glass draws fire from the hand; and the hand, which is then emptied of its own fire, becomes apt to receive it back from the overloaded glass; both when this escapes from the hand, and when it returns to it still loaded with excessive fire: but as this same hand, when the experiment is made with wax, itself becomes overloaded with fire, it then throws it towards the wax, as it moves away deprived of its own, and towards that same wax when this comes back to it, still deficient: and as the overcharged glass diffused before its excessive fire into the Chain, so now does the Chain throw its own into the wax which, by being rubbed, is, in consequence of its peculiar nature, become deficient.

153. But with regard to the light of *separation*, and that of *return*, they are made to exhibit a more precise appearance of a star, and of a brush, by performing the friction with a piece of gilt paper; with this observation, that the glass must be rubbed with the gilded surface, and the wax with the naked surface of such paper. When the friction is executed on glass, the edges of the paper exhibit









hibit a series of most conspicuous *stars*; and, on the contrary, throw most vivacious *brushes* when the experiment is made on wax. Likewise, if the point which the Chain offers to the glass, or to the wax, be sharper than usual, then both the appearances of a brush, or of a star, will be more striking.

154. I have hitherto spoken of the three lights jointly: I must now say something on the multiplication that may be effected of the lights of separation and return. When I rub with my hand, or with any other such body as does not touch in a continued manner the surface of the rubbed body, a light appears in all such intervals as are formed between those parts into which the rubbing body happens to be divided, for instance, between my fingers, if I rub with my hand. Now, certainly, all these lights are nothing else than lights of *separation* and *return* together: where the glass ceases to be in contact with one of the fingers, a part of the fire left by the said finger, gets back into it, and another part of the same fire throws itself into the next finger, with which the glass is again in contact. This observation on those lights, besides its necessarily belonging to the subject, is moreover useful in this, that it shews that in exciting the electricity by a friction, there is no necessity to perform the same with a continued surface.

155. Those operations being once made, there is scarcely any thing else to observe with regard to the light of *separation* in particular, but that its existence and vivacity correspond both to the capacity of the Machine, and the efficacy of the friction. If the Machine be insulated, the light of separation will appear for as little time only as will be sufficient for the fire in the Machine to be carried into the Chain; this done, the rubbing hand can no more afford to deposit fire on the surface of the glass; therefore no more fire can afterwards flow back from the same into the fingers; and thus there will be no more a light of *separation*. All which confirms the second part of the law.

156. With regard to the third part of it, it is rendered equally manifest by the light of *distribution*; or otherwise, by that star which appears on the point of the Chain, where the rubbed glass passes nearest. The existence and vividity of it correspond, both to

the capacity of the Machine to give fire to the glass, and to the capacity of the Chain to receive the same. Thus, if I rub the glass with the insulated cushion as in Pl. I. Fig. 8. we can hardly perceive for an instant, and even in a dark room, a most weak light upon the point of the Chain. If I rub with my hand, standing on the insulated Machine, the light will appear somewhat longer; but now it will be found perpetual, if I rub from the ground, and if, besides, the Chain also communicates with the ground: all this is because, in the latter case, the Machine can perpetually give, and the Chain perpetually receive fire. But however strongly I may rub from the ground, when the Chain is once insulated, the star will not appear upon it any longer than the short time which is necessary to saturate it. I increase the capacity of the Chain, by annexing to it one or more phials to be charged; then the star appears the longer as more time is become requisite to complete the saturation; and the vivacity of the same is continually and gradually decreasing as the saturation gradually draws nearer to its completion. Whence there can be no doubt but that the star is formed by fire which from the glass diffuses itself into the Chain.

157. All this is no less evident with regard to such fire as is accumulated by the friction, on that part of the glass which from the hand proceeds to the Chain: the following experiment will demonstrate it. Let the glass be revolved in the direction *abcd* (Pl. III. Fig. 2.) and the point of the Chain be placed near *b*. If, things being ordered so, I present a brush made of metal wire to any point *ab* of the glass, in that part of it which proceeds to the Chain, then there is no longer either star or other sign of electricity on the Chain; but if with the same brush I glide slowly along that part *c* of the glass which has already passed the Chain, then the latter exhibits, as usual, the star and other ordinary signs. If I make the glass turn in a contrary way, the same things will also take place when I apply, likewise in a contrary manner, the metallic brush. Universally, in order to have the star appear on the point annexed to the Chain, and the Chain charged in consequence of that star, it is requisite that the portion of fire on the glass do not meet, before it reaches the Chain, any other body into which it may diffuse itself.

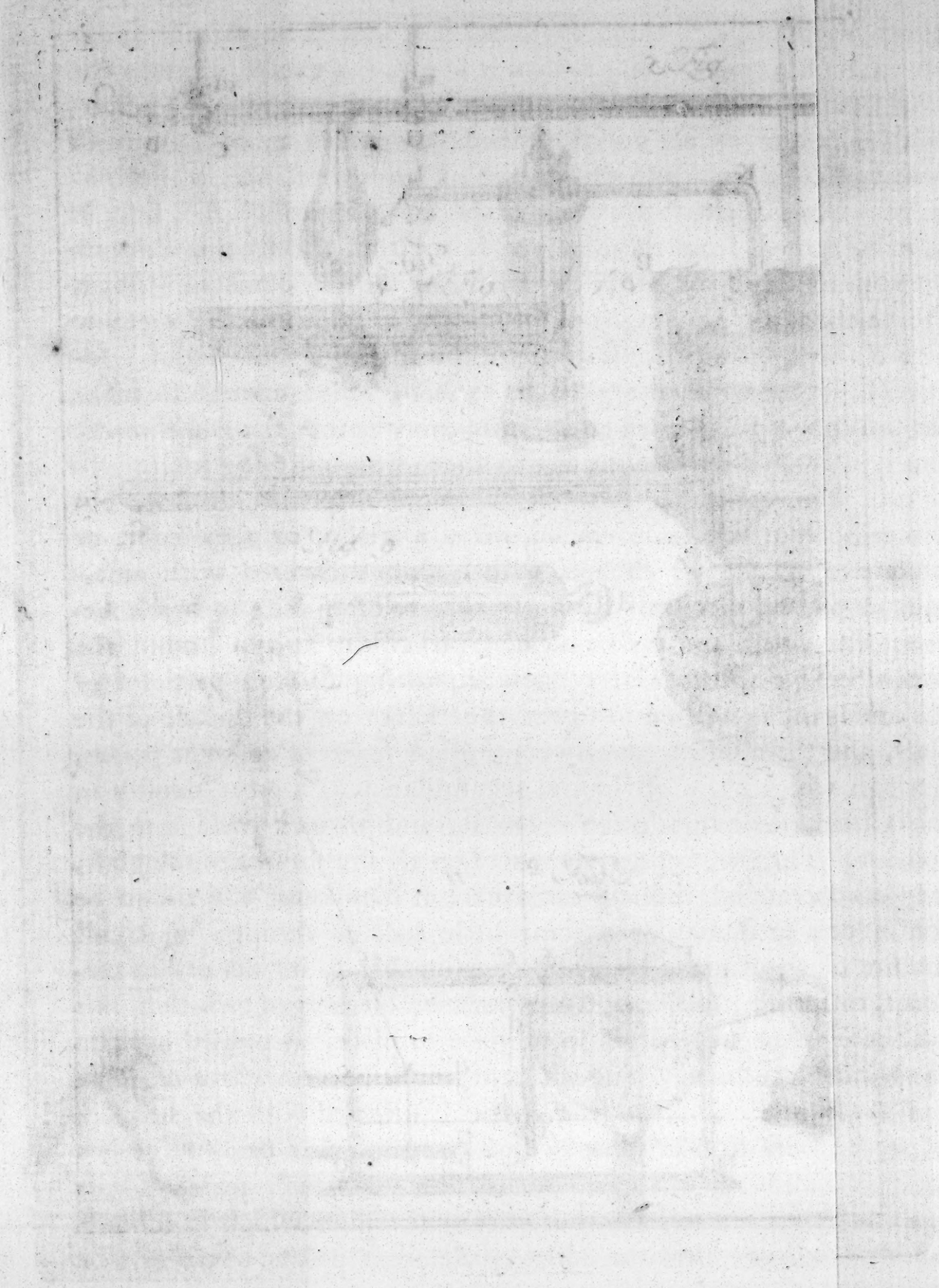
158. Lastly,

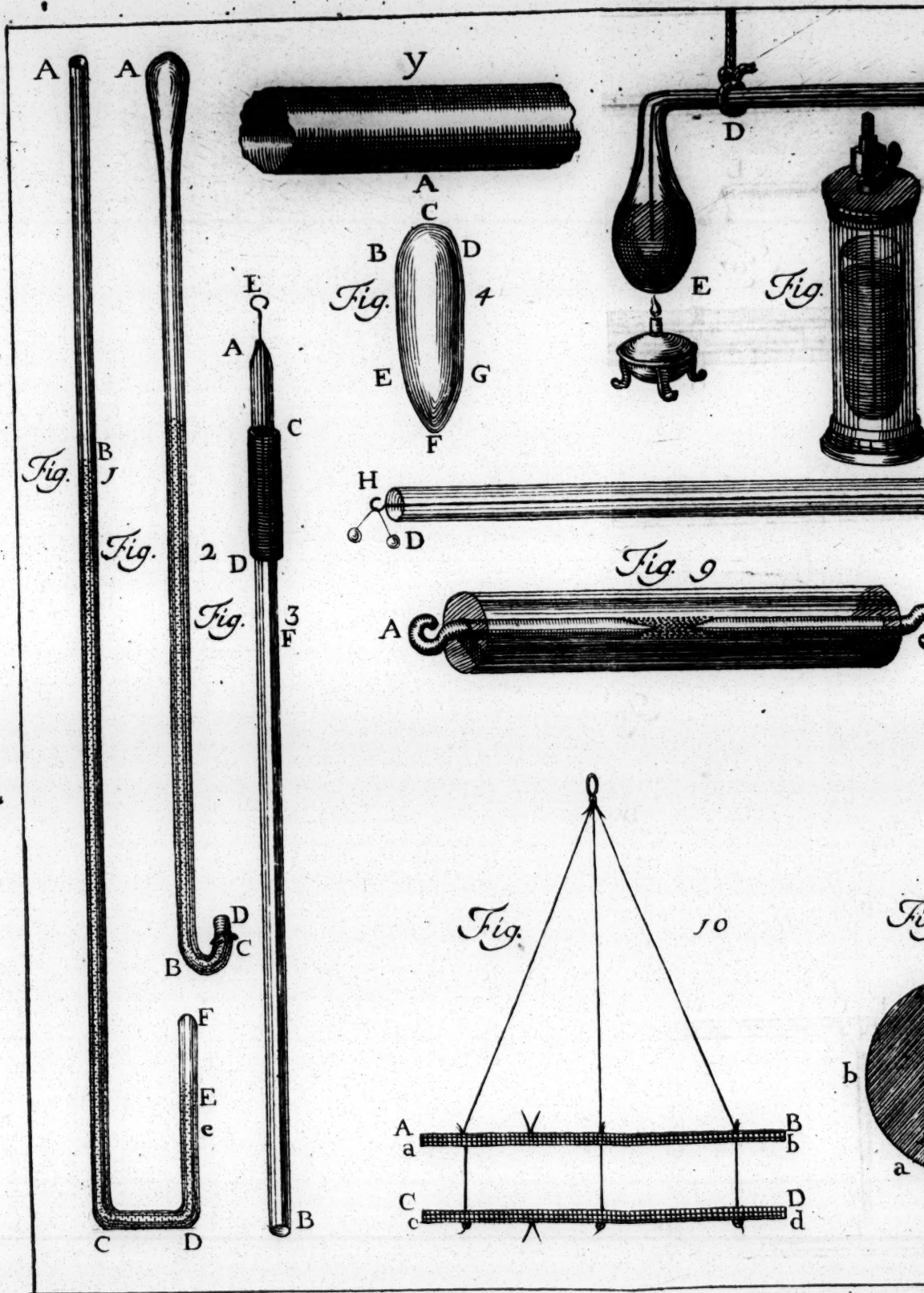
158. Lastly, the light of return corresponds, with regard to its existence and vividity, to the capacity of the Machine to give fire directly, and to the capacity in the Chain to receive it, inversely. That is to say, according as the whole, or only a portion, of the fire on the glass gets into the Chain, none, or a less part of it, remains on the glass there to form the light of *return*. Thus, if the Chain communicates with the ground, it then can admit any quantity of fire the glass may carry to it; consequently, little or no fire will remain on the latter to form the light of return. If the Chain be insulated, the light of return speedily returns, and soon increases its vividity, the Chain being then soon saturated with electrical fire: if, in the mean time, a spark is drawn from the Chain, then the star begins afresh to appear upon its point, and the light of return on the glass again fails. Let afterwards a large jar of glass be annexed to the Chain, in order to be charged; the light of return will then be continually increasing, in proportion as the star on the point of the Chain will be continually deadening: that is to say, the light of return will decrease in proportion as the charge of the Chain, (of which the capacity is become larger on account of the annexed jar) draws nearer to its completion. In short, the light of return, otherwise the fire remaining on the glass, when from the Chain it goes again to the hand, is to the light of *distribution*, that is, to the fire which from the glass goes to the Chain, as the capacity of the glass, is to the capacity of the Chain.

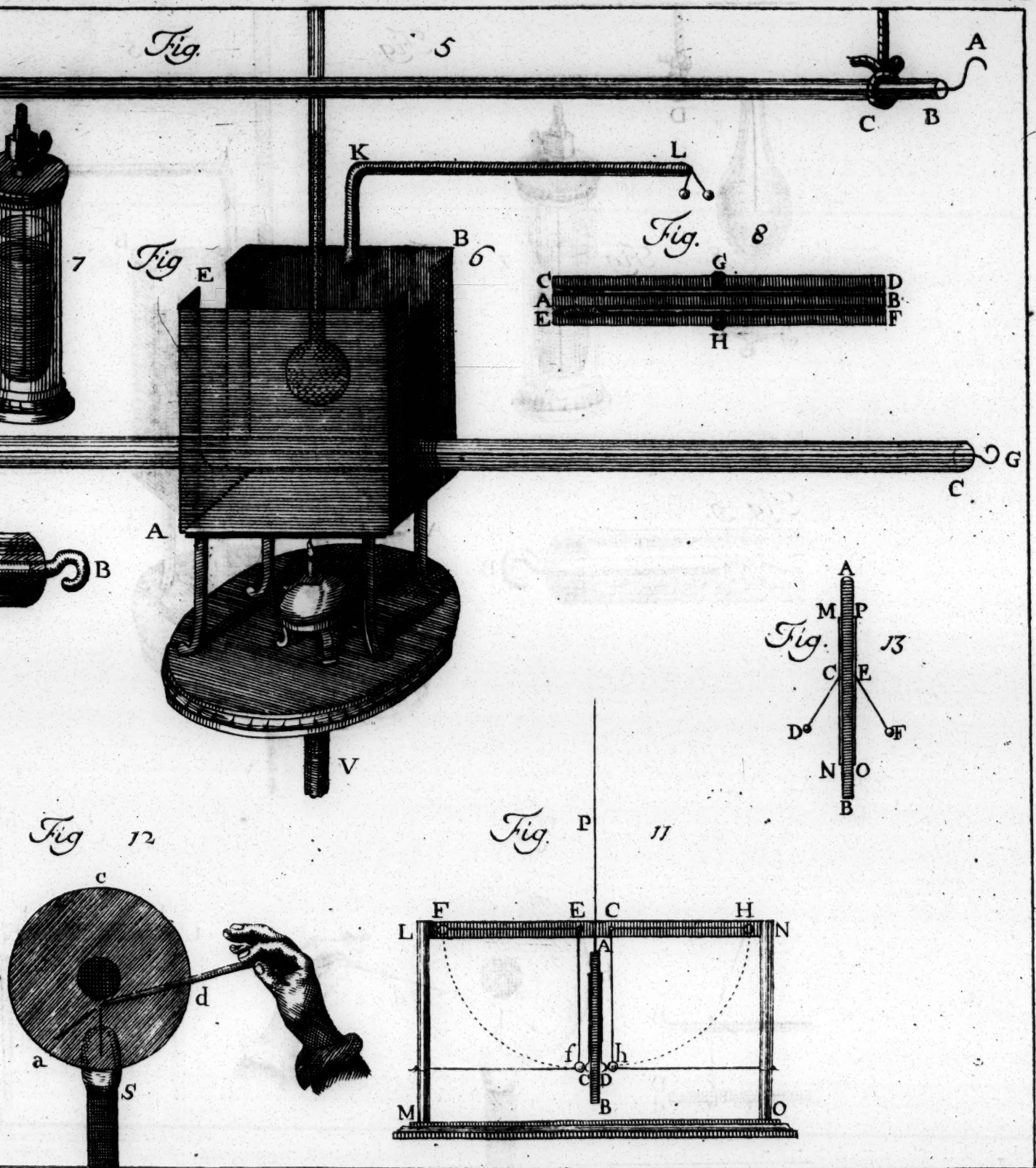
159. Those three lights therefore, viz. the light of separation, the light of distribution, and the light of return, demonstrate with the utmost evidence, the whole tenor of the two last parts of the law of *excitation*; but it is proper here to mention some accidents of that light itself, which might create some difficulty to new observers. The first is, that when instead of using the point of the Chain for exhibiting the star, I use a brush of metal fixed to it, then long and extensive brushes of fire spring from several of the threads or wires that compose it. But if we consider the circumstances and the directions of these brushes, we shall discover their real cause: they only spring out when the Chain is fully charged; they only spring from such wires, or threads, as may
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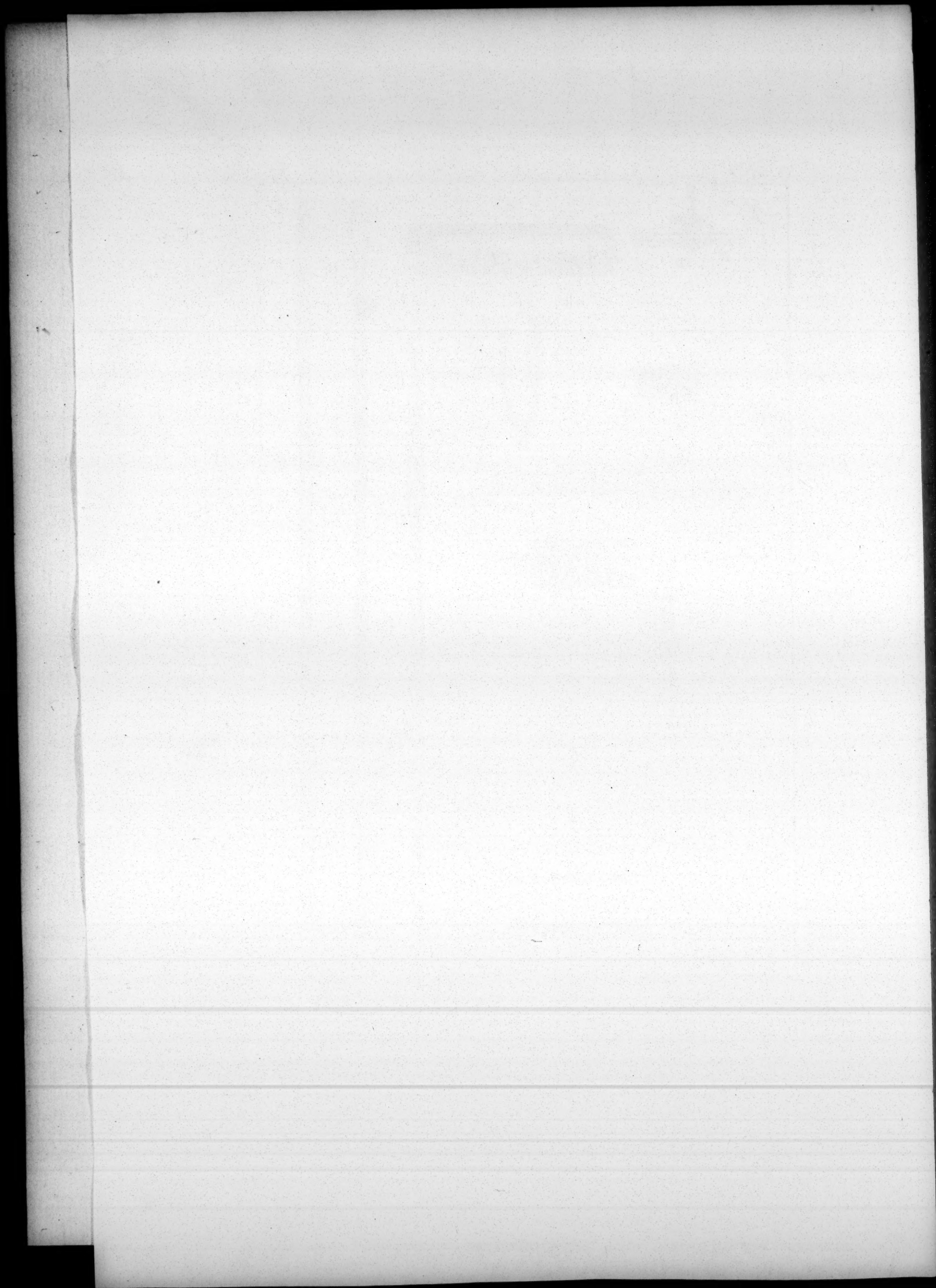
direct themselves towards the Machine, and these threads really direct themselves that way: that is to say, the excessive fire in the Chain directs the moveable parts annexed to it towards such neighbouring bodies as are not electrified, and much more so towards such bodies as are contrarily electrified, as for instance, is the Machine when insulated: no wonder therefore, that such fire, since it actually springs from those moveable parts of the Chain, exhibits the appearance of a brush. From all this we may conclude what is the best way of suppressing the said brushes; let first the metallic threads be kept fixed and directed towards the rubbed glass; secondly, let a part of the glass be rubbed, broad enough to make the fire rush from it, even laterally, to every part of the said threads, and it will thus restrain the fire in them from diffusing itself.

160. The other accident is still more surprising at first, but proceeds from the same cause: when a globe, or a cylinder, or a plate of glass, are rubbed with cushions covered with amalgam of tin and mercury, strong sparks are often seen to break between the Chain and the hand, and plentifully to run around the circumference of the glass: these are owing to such particles of the amalgam as happen to spread themselves on the surface of the glass, and there effect a pretty continuous series of deferent points, through which the electric fire, accumulated to a great density in the Chain, discharges itself from it, and throws itself into the cushion. The fire, besides, deposited on all the surface of the glass, may co-operate in the said accidents: it may cause a spark to be continued; or it may open some little passage through which all the fire of the Chain, which proportionably to its density endeavours to spread itself, may afterwards be discharged; so that this fire, being thus transported from place to place, is carried back to the hand or cushion. The accident might be productive of some bad consequences, if the friction being effected with the hand, a discharge were to take place in the very time that many large jars are just completely charged: in order to avoid such accident, care must be taken not to have that part of the glass which is rubbed, exactly polished; thus the series of deferent points never can be completed: besides, the hand must be kept at as great a distance as can be from the Chain; it even will be better to rub with a cushion.









APPENDIX.

161. Of two bodies whatsoever, A and B, so situated as to be easily brought to be rubbed, the one by the other, the one *v. g.* the body A gives to the other its natural fire and all other that may come and successively supply the defect of that which it has already given: in this manner, the excitation of two contrary electricities is preserved; viz. of an electricity by deficiency in A that gives, of an electricity by excess in B that receives the electrical fire. But why, in any given case, does the one of the bodies, *v. g.* A give its fire to B, rather than B gives its own to A? The solution of this question would be an excellent and most luminous complement to this chapter.

162. But in case we could not succeed in resolving that difficulty, I think it will not be amiss to present here some propositions that may at least procure some approximation towards the said solution. I observe, first, that *of two bodies of a same or similar nature, that which rubs gives electric fire to that which is rubbed.* I mean by the body that rubs, that which moves on a same part of its surface successively along successive different parts of the surface of the other; which latter, I consequently call the rubbed body.

163. The first experiment that led me to that important truth has been made on a stocking made of black silk, which I had closely wrapped around a cylinder of glass (Pl. III. Fig. 12.) and tied with strings of silk *ah*, *ab*, *cf*, *cd*, and stitched together in *ac*: I have rubbed it while the said cylinder was turning, with the fellow-stocking to it of black silk, when I have seen an excess rise in the Chain, and a deficiency in the Machine.

164. I soon after substituted a stocking of white silk to the black one, and rubbed it with the fellow to it, of white silk, when the same effect has resulted.

165. I substituted a band of black velvet, and in rubbing it with a piece of black velvet, I saw likewise sparks by excess take place on the Chain. I substituted a band of white velvet (all these bodies were applied after the same manner to the cylinder, Pl. III. Fig. 12.) and in rubbing it with a piece of white velvet, I perceived the same effects.

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166. I then proceeded to try if the same would also take place in making the experiment, as I have done so often, with the skin of a cat. Having applied this to the cylinder, I rubbed it with a piece of skin of the same kind, when I obtained sparks by excess from the Chain, as strong, at least as any I could excite by rubbing with my hand.

167. I have tried the same experiment with glass, by rubbing a cylinder of glass pretty solid, sometimes with a tube, and at others with a stick of glass; but in whatever manner I might press upon the glass, I never succeeded to perceive the least electricity arise in the circle traced by the tube or stick, on the cylinder while it turned; however, when I pressed more strongly, the two glasses then began to grind each other; and there arose, in the place of the friction, a little flame of a golden reddish colour.

168. I imagined that the friction in this experiment produced no electricity, because it was performed with too much force and destroyed the glass: I tried to moderate it. I fixed two sticks made of sealing-wax to the centres of two thin polished round plates of crystal, about five or six inches in diameter: then, with the help of these two sticks, I pressed the two uncoated surfaces against one another, and being thus rubbed, a degree of electricity resulted in both. When I afterwards attentively examined in what manner the friction had been performed, I constantly found that that plate which with its most prominent parts described the largest circles or lines on the surface of the other, grew electrified by deficiency, and the other by excess.

169. I then tried to experiment with sealing-wax. I took two sticks of a same size, and with a same part of the surface of the one, I moved along the sides of the other, which, at the same time, I was softly revolving on its axis: when I found that the stick with which I had rubbed the other, had contracted in the end an electricity by deficiency; and that which had been rubbed, electricity by excess.

170. Seeing how constantly the above principle obtained, when the natures of the bodies were *the same*, I tried if it would prove equally true with regard to bodies of only *similar* natures, by rubbing hair of a kind of animals, with hair of animals of another kind;

kind; and making use of hair of a certain fineness, I have always found the *rubbing* body to give its own fire to that which was rubbed*.

171. The result of those experiments is, therefore, that of two bodies, A and B, of a same, or very similar nature, A which in a given time undergoes the greatest friction, will give its fire to the other. Let us suppose the case mentioned above, of a band of black velvet wrapped around the cylinder, and rubbed with a similar piece of black velvet: the friction which the latter piece undergoes, is to that undergone by the former, as the revolving circumference covered by the said band, is to the arch occupied by the piece of velvet by which it is rubbed: so that if the revolving circumference is supposed to be $= 360^\circ$, and the said arch $= 15^\circ$, the friction suffered by the rubbing piece will be to the friction suffered by the rubbed band, as 24 is to 1; that is, when the band is rubbed once, the piece is rubbed twenty-four times.

172. But, trying afterwards to make experiments with substances of different natures, I found that the above law, viz. that the body which suffers the greatest friction, gives its fire to the other, obtained no more: *there are, on the contrary, certain substances which, even when undergoing a much less degree of friction than others, yet love to give their fire to them.* Of this kind are resins, sulphurs, oleous, oiled, or even varnished substances, and the various kinds of silk, especially those dyed black. When I rub with my hand, with paper, or with a piece of oiled silk covered with amalgam, a stick of sealing wax, or of resin, or of sulphur, or a stick of wood varnished with oil of linseed, let such friction be performed in any manner it will, the result will nevertheless constantly be, that those bodies, instead of giving fire to the hand or the paper by which they are rubbed, will draw the same from thence: and if the friction is performed on a ribbon of black silk, the effect will still prove the same, and the ribbon will also draw fire from the

* I had already made all the above experiments, when an experiment made after the same manner by Mr. Bergman, came to my knowledge (Phil. Trans. tom. 54. § 3.) He electrified by excess a white ribbon, by rubbing it, in its whole length, successively, against a given part of another white ribbon.

said hand or paper. Lastly, the same piece of glass which, as long as it continues polished, receives electric fire from other bodies, gives its own, when it is once ground with emery, and rendered rough; as Mr. Canton has first observed. I have a tube of crystal A B C (Pl. III. Fig. 13.) polished in A B, and rendered rough with emery in B C; with a single friction I electrify it by excess in A B, by deficiency in B C.

173. But with regard to the property of certain bodies, of giving their own fire, we may lay down this in some degree universal law, viz. *that metallic bodies have in themselves, in an eminent degree, this property, that when they are rightly managed they can give their own fire, and electrify by excess, such other bodies as would in other cases give theirs, and be electrified themselves by deficiency.* I apply to a Machine the cylinder of wood A B (Pl. III. Fig. 8.) encompassed all around with a very thick band of sealing-wax, or sulphur: then, with the same gilded card, I can electrify either that band, or the Chain, which ever I will, either by excess or deficiency; viz. if I rub with the naked side of the card, the Chain will be electrified by defect; if I rub with the gilded side, the same will be electrified by excess. Opposite kinds of electricity are also produced on a stick of sealing-wax, by rubbing it alternately with the naked side of the card, and with that which is gilded or silvered. If I afterwards rub the said stick of sealing-wax, with the silk cloth covered with amalgam, the wax becomes electrified by excess, with as much vivacity as it ever is electrified by deficiency, when rubbed with bodies of another kind. Likewise, however a black silk ribbon may love to give its own fire, yet when rubbed with the silk cloth covered with amalgam, it acquires a considerable degree of electricity by excess: even the black velvet, even the unpolished glass, when rubbed with the amalgam, become electrified by excess. And here we must mention again the usefulness of the amalgam in procuring a strong electricity by excess in bodies, such as glass and polished crystals, which, of themselves, are so much inclined to it. To the same cause I impute the strong electricity I can excite in the Chain, by turning the plate A B (Pl. I. Fig. 10.) in mercury (§ 35.) And if the plate might be turned with a greater degree of velocity,
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without scattering the mercury, I should prefer that method of exciting electricity to any other: thus all trouble with regard to ordering the cushions and keeping them clean, would be avoided, as well as the necessary care to make them press against the plate sufficiently and uniformly; which circumstance I do not know if it ever can be, after all, so well obtained as it is with mercury; which, in consequence of its weight, continually presses against the plate.

174. I shall finish with relating how by rubbing with a metallic surface, I have changed into an electricity by excess, the strongest electricity by deficiency that ever was excited. I procure the latter electricity by rubbing a band of black velvet, applied round the cylinder (Pl. III. Fig. 12.) and entirely freed from moisture by the help of the skin of a wild cat, made very warm; I obtain from that velvet an electricity by deficiency in the Chain, as strong, at least, as any electricity by excess I can raise in it by the help of the cylinder, globe, or plate; which, by the by, shews that the electricity by deficiency does not differ only in degree of intensity from the electricity by excess. But afterwards, to return to our object, if I rub the same band of black velvet with the amalgamated cloth, that powerful deficiency in the Chain is gradually turned into a pretty considerable excess.

175. I shall propose here some questions. May not we impute such a propriety in metals, of giving their own fire, to their being deferent in the greatest degree? May not we impute to the same principle the increase of efficiency in the cushions or fingers that rub the glass, when they have been previously made wet, and that an humid vapour, (as being deferent) like metals, increases electricity? We shall see afterwards that the common fire propagates itself with more ease through such bodies as conduct easily the electric fire, than through such as possess that quality in a less degree; and that the electrical fire produces with the greatest rapidity those very same effects which the common fire is used to produce slowly. May it not be that those two kinds of fire only differ in their degrees of purity; that is, that the electrical fire is free from those particles of other substances within which the common is in a manner imprisoned? A rubbing body contracts more

heat than a rubbed body; may not the reason be that the body that rubs actually diffuses its own fire into the other? I once repeated the ingenious experiment of Mr. Bergman, by moving the same part of a white ribbon, not warmed, along the surface of another white silk ribbon, stretched over lighted charcoal, and consequently warmed to a great degree, when the rubbing ribbon, which was cold, became electrified by excess; the rubbed ribbon, which was warm, became electrified by deficiency. Is not the inflammability of resins, sulphurs, and other oiled substances, the cause why such bodies are inclined to be electrified by deficiency, since in consequence of such inflammability they receive, when rubbed, a greater degree of heat? And may not the great quantity of phlogistic with which metallic substances abound, as well as the great ease with which common fire moves within them, be the reason why metallic substances quickly contract by a friction, a considerable degree of heat, and are thus disposed to give their electric fire in the highest degree? We shall see that when metallic bodies are melted by lightning, the bodies near them are much exposed to be burnt by the phlogistic which is then freed out of them: we may add to the above conjectures, that glass, when unpolished, also contracts heat more easily on account of the separation between the parts which compose its surface, and of their smallness; in the same manner as inflammable bodies are the more easily kindled, as their surface has a greater ratio to their mass.

SECTION H.

On the Theory of insulating bodies, with regard to the charging and discharging of them.

176. **I** Confine the subject of this section to the charging and discharging of insulating bodies, as the treating of those different operations will be somewhat long; and besides, I must treat separately of the particular property of the *vindicating* electricity; a subject which I have amply explained in the book entitled, *Experimenta atque observationes quibus electricitas vindex constituitur, atque explicatur*. Taurini, 1769; nor can I give a convenient abridged exposition of that property, before I have treated of the different kinds of electrical motions.

C H A P. I.

In which the theory of insulating bodies, with regard to the charging and discharging of them is explained, and proved by the help of the brush and little star, on plates of glass especially; and is extended to pieces of glass of various shapes, and to other insulating bodies.

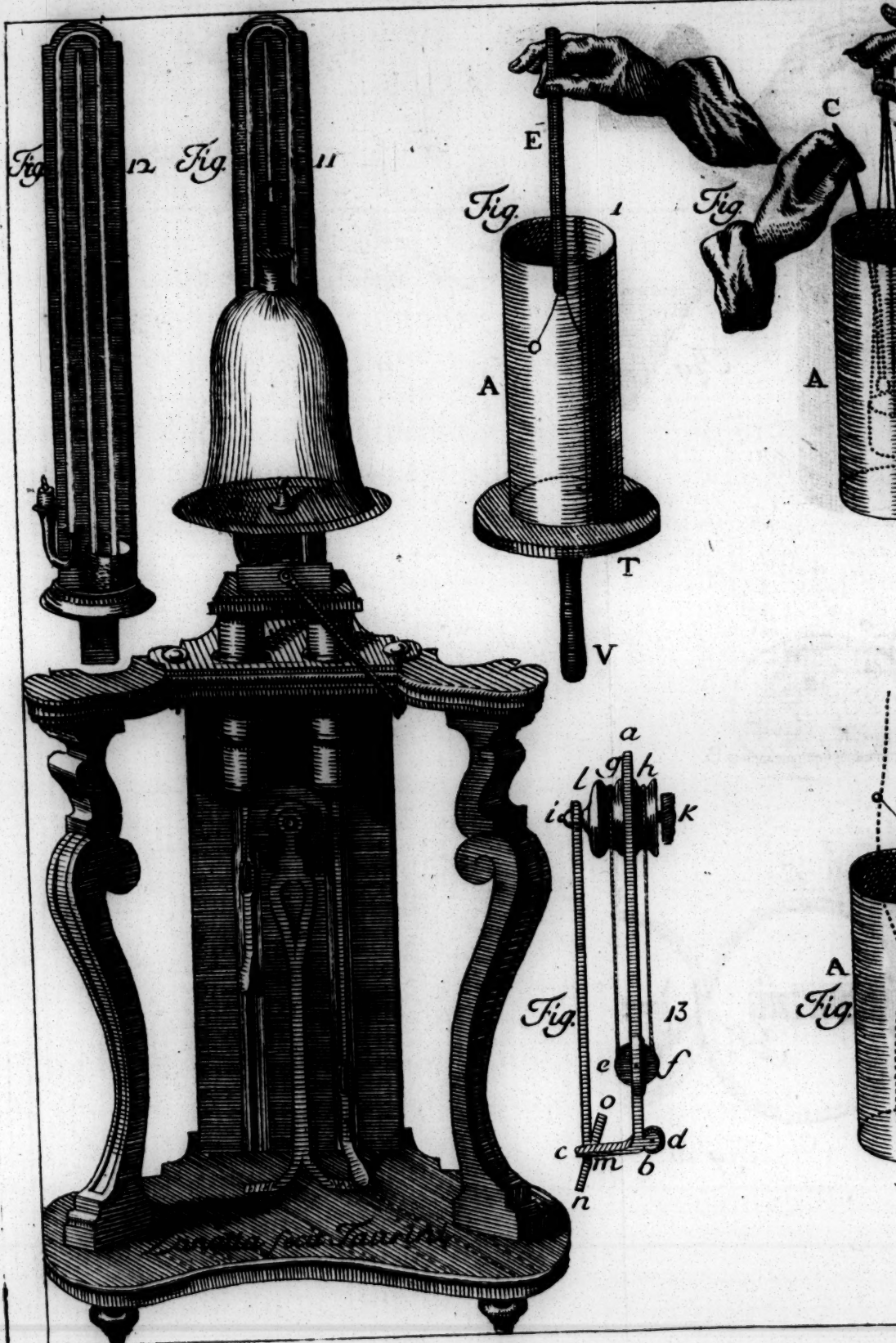
177. **I**N the year 1746, Mr. Muschenbroek endeavouring to increase the electricity of deferent bodies, by surrounding them with such bodies as might most exactly insulate them, put some water (a body which is deferent to a considerable degree) into a bottle of glass; the latter body being much more compact than the air, was consequently much more capable of insulating the water: then the said gentleman formed, with a rod of metal, a communication between the Chain and the water in the bottle; he then grasped the bottom of the bottle with one of his hands, and no sooner had he touched the rod which from the Chain was let into it, in order to remove the bottle, but he received a strong shock, which was accompanied by a loud spark, which rushed to his hand from the said rod. Such is the origin of the denominations,

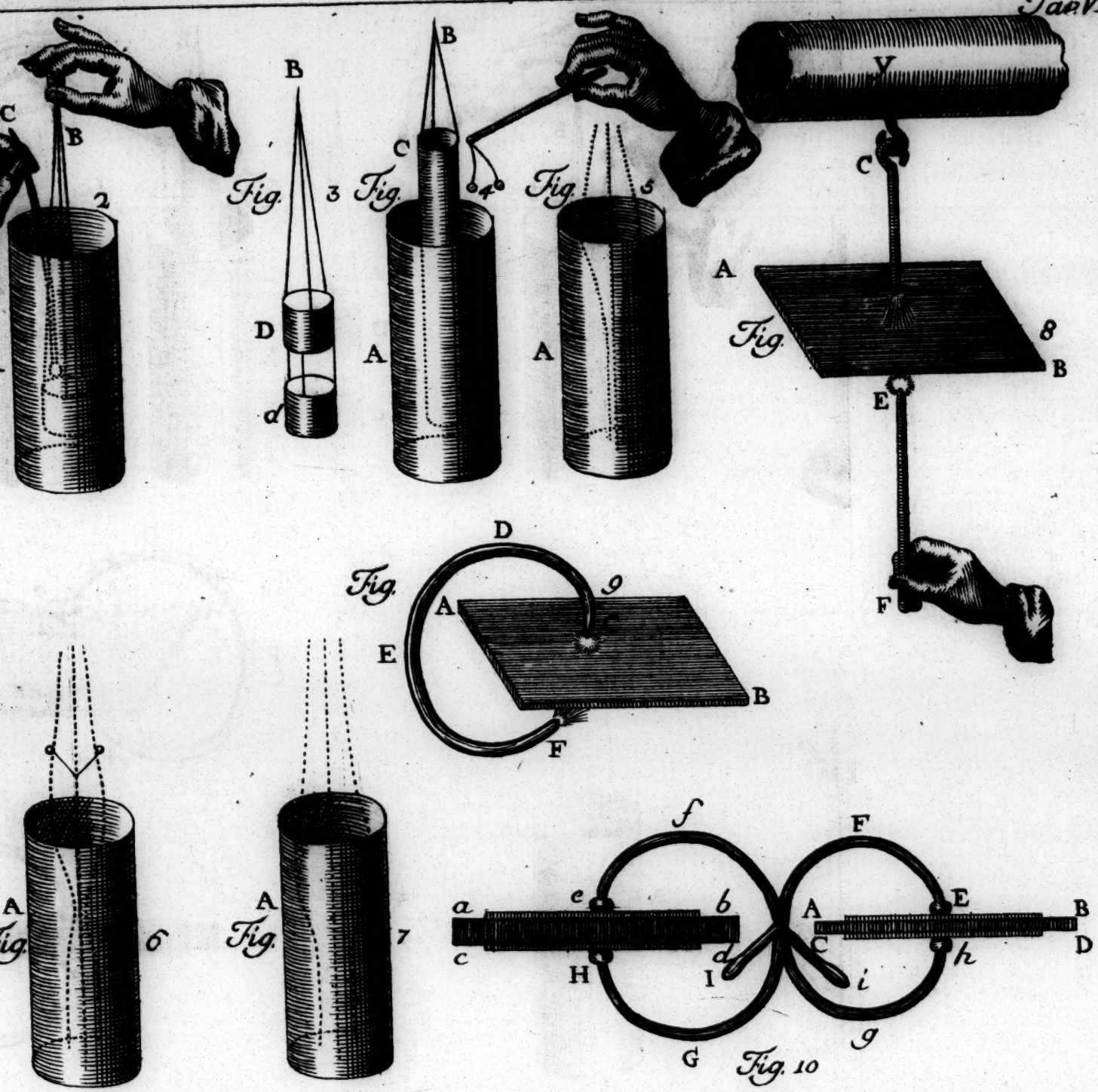
tions, *experiment of Leyden*, and *bottle of Leyden*: since that time other phrases have been used, as of charging glasses, that is, of introducing into them a strong dose of electricity; and of discharging them, that is, of taking from them the introduced electricity.

178. No later than the year 1747, Dr. Benjamin Franklin analysed that experiment; and here is the substance of the theory he laid down. *I. A body, in order to its being capable of being charged, must be insulated, impenetrable, sufficiently ample, and sufficiently thin.* This body must be insulating, that is, of such nature as not to allow the electrical fire to move from one place of it to another, either along its surface, or through its substance: it must be impenetrable, that is, besides its being insulating, it must be compact enough to keep the electrical fire from making its way from one of the opposite surfaces of it, to the other, either through the interstices of the rare texture of it, like that for instance of a silk cloth; or through any small fissure; or also through a mixture of deferent particles that might form on its surfaces a sufficiently continued series. Again, such body must be sufficiently ample, as all other circumstances being equal, the quantity of the charge on a body must be proportioned to its size. And it must be sufficiently thin, because all other circumstances being again supposed equal, the quantity of its charge will be proportioned to its thinness; though, it must be added, that such thinness must not be neither so much to the detriment of the necessary solidity, as to allow the force of the charge to break the too weak substance of the body.

179. *II. In order to produce the charge, an ample communication must be established between the two opposed surfaces of the insulating substance (taking care that one of the said surfaces communicates not with the other) and two different systems, the one of which, at least, must be actually electrified.* Because, the quantity of the two equal contrary electricities that will take place on the two opposed surfaces, and which constitutes that which we call the *charge*, will be proportioned to the quantities of the contrary electricities raised in the two systems jointly; or, if an electricity is excited in only one of the two systems, to the quantity alone of that electricity. Let DC represent the profile of a large plate of glass (Pl. IV. Fig. 1.) let FaE represent the communication between
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one surface of the same and the Chain, and *H b G* the communication between the other surface and the Machine. The excessive fire which the rubbed glass transmits to the Chain, will be accumulated in a great quantity on the surface *F a E*; and, though it does not penetrate through the substance of the glass, will drive an equal quantity of natural fire from the other surface *H b G*. And, reciprocally, the force of the cylinder, by its drawing a great quantity of fire from the Machine, will consequently draw a great quantity from the opposed surface *H b G* of the plate, annexed to it; and, at the same time, enable a great quantity of excessive fire to run from the Chain to the surface *F a E*: the concurrence of those two forces will thus accelerate the formation of the two contrary electricities, and augment their quantity in proportion as the natural fire will be enabled to escape from one of the surfaces, and the excessive fire to accumulate itself on the other. If one of the surfaces v. g. *F a E* communicates either with the Chain, or with the Machine, and the other surface communicates with the ground, (which is the common way of making the experiment) the excessive fire that will flow into *F a E*, will suffice to drive the natural fire from *H b G*; or the natural fire that is extracted from the surface *F a E* (in case it is made to communicate with the Machine) will suffice to enable a quantity of excessive fire to run into *H b G*, but the operation will in that case be slower and less efficacious, just as we have seen, in § 122, the brush direct itself with a much greater force from the Chain to the Machine, than from the Chain to indifferent bodies, or from indifferent bodies to the Machine.

180. I must add, that a communication of the two surfaces with two different systems is absolutely requisite: *because the excessive fire cannot accumulate itself on one of the two surfaces, without the opposed surface communicates with a system into which it may, in the mean time, transfuse its own natural fire: nor can one of the surfaces transfuse its own natural fire, without the opposed surface communicates with a system from which excessive fire may flow into it.* That is, the charging of an insulating body is effected by raising at once, and in equal degrees, the two contrary electricities on the two surfaces of the said body; either through the action of
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two systems animated by contrary electricities, or through the action of an animated system on one surface, while a communication remains established between the other surface, and the indifferent system.

181. III. *Lastly, in order to effect a discharge, a communication must be introduced between the two surfaces of the insulating body, when loaded as above with equal degrees of contrary electricities.* This is because the two equal contrary electricities are no less dependent on each other, with regard to their being destroyed, than as to their being produced: that is, as a quantity of excessive fire cannot be introduced into one surface, but inasmuch as an equal dose of natural fire can quit the other surface; so *vice versâ*, a quantity of excessive fire introduced into one surface, cannot be taken off, but inasmuch as the natural fire taken from the other, is restored to it. Consequently, as soon as a communication shall be completed between the two surfaces of the insulating charged body, by the means of one or more continued deferent bodies, the excessive fire will burst from that surface on which it was accumulated, and rush to the other surface which was deprived of an equal portion of natural fire; and thus the insulating body will find itself discharged, that is to say, will find itself with its two surfaces restored to their natural quantity of fire.

182. In short, the effect of a charge is to make the natural fire of the insulating body change its position without changing its quantity; any portion that may be added to one surface, is constantly taken from the other; the fire in consequence of the discharge does not decrease, it only returns to its natural position.

183. Hitherto I have only had for my object to give an abridgment of the theory; I am now going to demonstrate it, by the help of the signs of the *brush* and the *star*, as being the most proper to render it evident. I. I hold by one of its angles, a plate of glass A B (Pl. VII. Fig. 8.) which must be thin and dry, and at first only present it to the brush D, which springs with vivacity from the rod C D annexed to the conductor T; when the brush very soon ceases and disappears: that is to say, the electric fire thrown at first by such brush, cannot flow along the surface of the insulating glass, nor go through its compact substance and get at
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my hand; nor again adhere to the surface of the glass but in a small quantity, equal to such portion as it may drive from the opposed surface of it, into the contiguous air.

184. II. But afterwards, if I present to the inferior part of the plate another rod *EF*, standing in the same line with the point *CD*, the brush then revives on the point *D* of the rod *CD*; and, corresponding to it, the star appears on the point *E* of the rod *EF*: that is, the excessive fire of the brush is now enabled to diffuse itself along the superior surface of the plate, and then unite with the natural fire in it; because the natural fire of the inferior surface now can pass into the deferent rod *EF*, and through it into the ground.

185. III. But it is to be observed, that if the two rods remain directed to the same part of the plate of glass, the star and brush soon grow languid and fail. In order to revive them I must bring the rods nearer to the plate; but although they may at last become so near as to touch it, if they still remain directed to the same part, all light will cease to appear on the points. Whence it follows, that given portions only of electrical fire can be added to, and taken from, given opposed parts of the plate. One surface resists the more against receiving new fire, as it has already received more; and the other surface resists the more against giving its fire, as it has already given more: all which, if it is attentively considered, will be found conformable to the law of *distribution* (§ 86.) it is therefore very natural that while the points remain directed to the same opposite parts, the brush and star, that is, the fire that flows to one surface, and that which leaves the other, should first decrease and then fail.

186. IV. But if I move the plate horizontally between the two points, so as to make different parts of it successively correspond to them, the brush and star again appear, grow languid and successively fail; and, in short, exhibit the same series of appearances as before, relatively to all such new parts of the plate as are successively presented to the points.

187. V. If in moving the plate, I bring it to such a position between the points as to make the latter be very near the edge of it, then the brush of the point *CD* bends itself around this edge, as

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if it sought to avoid the glass; and from the edge bends itself again towards the point *E F*: that is, the electric fire declines going to the plate, and directs itself immediately to the other deferent point.

188. VI. If, after having by the means of the brush and star, strongly charged the greatest part of the surface of the plate, I remove it from the rod *CD*, and present to either of its surfaces the point *E F*, the plate is not for all that discharged: for as the other surface remains insulated, this surface cannot diffuse away the fire with which it is loaded; it can do it only when the natural fire, of which the latter is deprived, shall be enabled to return to it.

189. VII. If, after having charged the plate by the means of the brush and star, I present to the one surface of it, one end *C* (Pl. VII. Fig. 9.) of an incurvated metallic rod *CDEF*, and to the opposite surface, the other end *F*; then the plate is discharged, that is, loses the fire of its superior surface, which fire then forms a star on the point *C*; and, after circulating through the rod *CDEF*, forms a brush on the other point *F*; thus restoring to the inferior surface the portion of fire that had been taken from it.

190. VIII. The law of the two signs appearing on the two ends of the rod bent towards the two surfaces of the charged plate when it is discharged, is the same (except that the signs appear in the contrary places) as the law of the signs manifested on the two points *CD*, *EF* (Pl. VII. Fig. 8.) when the said plate is charged. That is, the star does not appear on the end *C* (Pl. VII. Fig. 9.) but as long as the brush appears on the end *F*; nor does either of them appear but as long as the ends *C* and *F* are directed to the correspondent parts of the two opposed surfaces. And with regard to given parts of those surfaces, these signs gradually, and pretty quickly, grow languid and then fail; though they revive when the ends *C* and *F* are successively presented to other parts that have been also charged; and, lastly, with regard to each several part, the vividity of those signs destructors of the charge, corresponds to the vividity of the contrary signs with which the charge has been introduced into that part.

191. I repeat the experiment of introducing the charge into the plate *AB* by the means of the brush and star; (Pl. VII. Fig. 9.)

then I place on the palm of one of my hands a sheet of leaf-tin, like those used for looking-glasses; and, holding the plate by one of its angles I invert it, placing it upon the sheet. I then lay on the surface of the same hand another sheet, equal to the first, and place it upon the upper surface of the plate. In short, I apply the two sheets in such a manner to the two surfaces of the plate, that there be no communication between them; and consequently, one surface only communicating with my hand, no discharge can happen (181.) But the spark bursts out and gives me a shock in the instant that I carry my other hand to the sheet, placed on the upper surface of the plate. Then it is that the fire, introduced by the brush in the several parts of the under surface of the plate, bursts out at once, by the help of the deferent metallic sheet applied to it, and runs through my body to replace at once into the upper surface, by the means of the other metallic sheet, the fire that had been extracted from it.

192. And this shock which I receive from the plate clothed with the two metallic sheets, and charged with the brush and star, surely allows of no doubt that the electricity which produces it, does not lie in the deferent bodies joined to the surface of the insulating bodies, but entirely in the surfaces of the latter; since both metallic sheets had had, through my body, a communication with the floor, and could not consequently have any electricity; it therefore remains, that they only transmitted the electricity introduced before into the plate, when naked.

193. It has only been in order to render the theory of the charge and discharge more manifest, that I have begun with proposing the experiment of charging, and afterwards discharging, every successive part of the naked plate by the help of the brush and star; but in the common way of experimenting, there is no necessity for repeating these tedious operations. To both surfaces of the insulating body deferent bodies need only to be adapted; these will completely spread the contrary electricities all over the surfaces, when the charge is forming; and in the discharge suddenly and completely gather the said electricities. Thus, for instance, to the plate A B C D (Pl. II. Fig. 8.) let a sheet of tin *f g h i* be affixed with a proper kind of glue;

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let also a margin of two or more inches be left between the edges of the sheet, and those of the plate, on all sides; let a sheet of tin, equal to the former, be adjusted to the corresponding parts of the other surface; and such deferent bodies thus applied will at all times charge and discharge the plate, and are called the *coatings*.

194. In the present figure, the plate of crystal A B C D is raised on a round flat piece of wood, insulated by the little columns of glass K L. When there is an occasion for it, I place on the ground the iron boxes M, N, O, I, with hot ashes in them to preserve from moisture the naked margins left on the plate. Then I form a communication between the Chain and any part of the coating *if g h*; and, in order that the fire of the Chain may be transported to the superior surface, I form a communication between the ground; or, what is still better, between the insulated Machine (179) and the *coating* of the inferior surface: and thus, in a little time, a great deal of excessive fire is accumulated on the superior surface of the plate, while an equal quantity has ran away from the inferior surface: that is to say, the charge is completed. In order, afterwards, to effect a discharge, I make use of a rod of brass O N M, terminated by two balls, likewise of brass; I then bring the ball O into contact with the inferior *coating*, and when I present the other ball M to the superior one, then a strong spark (proportioned, all other circumstances being equal, to the extent of the *coating*) is thrown out. I call the brass rod, from the office I make it perform, the conducting bow; and a plate, thus armed or coated, is usually called with us, the *Franklinian square*; it being Dr. Franklin who first used one of such a size to imitate the effects of lightning.

195. But in experiments that do not require strong sparks, there is no necessity of using such ample plates, which cannot be charged but after a considerable time, and are not so manageable: I find that small bottles of thin glass with a long neck, are extremely convenient; they are inwardly armed by being filled with water, or small shot of lead; or by being coated inwardly with the amalgam we have mentioned before; the outward surface is coated, as usual, with a sheet of tin. I have represented two such

such bottles, insulated on the little stool S (Pl. II. Fig. 6.) and four others armed with glass handles, in the Fig. 3, 4, 5, 6, of Pl. III. The figure 5 of the latter, represents a bottle which is charging by receiving a brush *c* from the Chain, and forming a star *c* on a point presented from the ground B; and the 3d figure represents the same bottle that is discharged in forming the two same signs in contrary places, on two points that communicate either with the ground, or between themselves. The 6th figure expresses a bottle armed with points, which receiving fire from the Chain in *c*, there exhibits a star; and in C gives the brush to the ground, or the Machine; and the 4th figure represents the same bottle when it is discharged.

196. The law of the brushes and stars is still more distinctly exposed in Pl. IV. Fig. 8. On the little bench O N Q P a point D is placed; and, at some distance, a little column of glass is raised, which insulates the large plate F F, armed with the two coatings H G, K I: the plate presents two points B, C; the one to the little bench O N which communicates with the ground; the other to the conductor Q from which another point is directed to the coating. Now, at the same time that the plate takes its charge from the Chain, the excessive fire, which runs to the surface H G, forms a brush, as it comes out of A; and a star as it gets into C: and the natural fire that is thrown away from the opposed surface K I forms the brush in B, whence it issues; and the star in D, where it enters. But if after having completed the charge, I touch with one hand the conductor Q, and with the other the little bench O N, then the direction of the fire is inverted in consequence of the communication which I introduce between the two surfaces differently electrified (it is necessary, in such case, that the points should be placed at a less distance than that indicated in the figure) that is to say, every sign becomes inverted. If, in order to charge the plate, its surface H G were presented to the Machine, the signs, in the act of charging, would be contrary to the signs that appear when the charge is taken from the Chain, and would be the same as those manifested when a discharge is made of a plate charged from the Chain; but if that same plate were now charged from the Machine, a similar inversion

sion of the signs would take place, whenever it should be discharged.

197. Any person who will repeat the experiments of charging and discharging naked or coated glasses by the means of the brush and star, and will proceed with the necessary care and patience, shall be convinced by his own eyes of the truth of the proposed theory. I flatter myself that I have sufficiently demonstrated the significancy of both the brush and star in Chap. V. of the preceding Section, I intend now to demonstrate that experiment which I had reserved for the last part of this chapter; that is to say, how other insulating bodies (when sufficiently thin and compact) may be, like the glass, charged and discharged.

198. In the year 1753, I charged a piece of native talc. I took a sheet of it (Numb. 472 of the *Artificial Electricity*) as smooth as I could find it, transparent in all its parts, and free from furrows, or any opaque vein; I coat it in the same manner as a little Franklinian square, and charge it in the same manner. When I touch one of its surfaces with one of my hands, and the other surface with my other hand, a discharge is effected, and I receive a small shock.

199. I have spread sealing-wax upon a piece of even polished marble, anointed with oil of olives, and heated underneath by a fire not very violent (Vide Numb. 149. of the *Terrestrial Atmospheric Electricity*); the sealing-wax was spread in such a manner as to form a thin even coating on the marble without any interruption or crack: I then was able to charge and discharge it, in the same manner as the Franklinian square.

200. In Numb. 151 of the same book, I was struck in the same manner by a coating of sulphur, similar to that in the preceding paragraph. In Numb. 155 I was struck, though less strongly, by a coating of pitch alone. In Numb. 156, I was struck by a coating made with a compound of equal parts of rosin and pitch. In those experiments I not only convinced myself that all insulating bodies possessed the same propriety as the glass, but made besides, two observations. I. That though sulphur and resins receive from friction an electricity different from that of glass, however, when they are electrified by communication, they

they are electrified in the same manner. II. That the least crack, or fissure, is enough to hinder the charging of the above mentioned coating. I had armed, with a gilded card, a coating of sulphur, and could only obtain from it a very small spark; but then, when I was forming the charge, I heard, from time to time, a spark that leaped from the one surface to the other; having found out the place, I cut the gilded card around the crack, which remained uncovered, and then the spontaneous sparks ceased, and I could obtain the convenient charge.

20. I increased afterwards the effects of those experiments by increasing the size of the instruments I made use of, as is mentioned in the collection of my new experiments, sent to the Royal Society the 14th of January, 1766. The Fig. 7. Pl. II. represents my fulminating table, described in the said collection. I. I cover the surface of the table with a sheet of lead, so as to have three inches of margin left marked; only a piece of the sheet gets out in E, and is bent towards the side of the table. II. To the four sides of the table, I fix four rules, which are raised above it about the twelfth part of an inch; so that there remains an empty parallelepipedal capacity, having for its base the surface of the table, and for its height the twelfth part of an inch. III. I melt a certain quantity of resin extremely well refined, and mix with it an equal weight of powder of marble, thoroughly heated, to exclude from it air and moisture. IV. I pour on the table this liquefied mass, and rolling a cylinder on the abovementioned rules, I level the surface and make it even: if, in cooling, some cracks or fissures are left, I close them with a hot iron. Lastly, to the surface A B of that substance, I adapt the sheet of lead *a b c d*, distant likewise three inches from the margin on all sides; and thus I have what I call the fulminating table; because if I make the metallic coating *a b c d* communicate with the Chain, the table becomes most strongly charged; and when I apply one head of the conducting bow to the part of the inferior plate of lead that gets out in E, and present the other head to the superior coating, a spark flies out more vehement than any thrown by a plate of glass of an equal size. Such tables as this may be made of all sizes, and have this advantage, that resins drawing less moisture than

than glass, they are more completely insulated, and may be sufficiently charged, even in damp weather; but then they have this disadvantage, that through the vicissitudes of warm and cold weather, they become cracked in a thousand places.

202. I have obtained still louder sparks from the fulminating globe, represented in Pl. IV. Fig. 5. To construct it, I take, in the first place, a large ball, made of plates of sheets of brass, two feet and two thirds in circumference, and this ball, or globe, serves as an interior coating; then I cover the circumference GCBAEFGH, with a stratum of resin mixed with powder of marble, and this stratum is in its turn covered or coated all over the space CDABC with a sheet of lead; so that the body of the ball made of sheets of brass, which serve as an interior coating, remains unaccompanied by the exterior coating, in all the space occupied by the zone of cement EFGHAD C, which remains naked; I suspend the ball by the hook HI, which I make communicate with the Chain, and after charging it, I discharge it by applying one head of the conducting bow MLK in M, to the sheet of lead, and bringing the other head K to the hook I. The few sparks I have obtained from that sort of apparatus were extremely loud; more so, I think, than could be obtained from glasses of an equal dimension; but I have indeed drawn very few, every one of them considerably damaging the cement in the place M whence I drew them, and the heat afterwards of the summer has made it melt and run. The surface of the exterior coating of that ball, was about nine feet and a half. Besides its size, the shape of that ball, and some other particular circumstances that I shall mention hereafter, have contributed to increase the charges; but I shall speak more explicitly on that subject in its proper place.

C H A P. II.

In which the same theory is confirmed by simple charges and discharges, made in different ways.

203. **C**OMMON simple charges and discharges suffice, if duly attended to, to demonstrate the manner in which the same are effected. I lay hold of the larger part of one of the bottles represented in Pl. II. Fig. 6. and apply the hook of it to the Chain; when I see the electroscope annexed to the Chain diverge but slowly, and the more so, as the size of the bottle that is charging, is larger. When the electroscope has attained its greatest divergence, I present my other hand to the hook A of the bottle, and I draw a spark and receive a stroke, which are both the greater as the capacity of the bottle is greater; that is, according as more time has been spent in charging the same, and procuring to the electroscope its greatest divergence. Now, if the excessive fire had penetrated through the glass, it would have, as I held the glass in my hand, diffused itself through my body into the ground, and the discharge would not have had in the issue an intensity proportioned to the time employed in charging; that is, to the sum of fire that had been able to flow from the Chain during that time.

204. If, after charging the bottle, I place myself on an insulated stool, I am likewise struck, and there remains not either in the bottle, or in my body, any electricity; except perhaps a little remain of charge, otherwise an overplus of electricity, in the one of the coatings: of which accidents I propose to speak more at large hereafter. Now, a given quantity of electricity never is suppressed by its passing into a finite capacity, but as it finds there an equal quantity of a contrary electricity; but there could be no such electricity in my body, since during the charging it communicated with the ground. The only conclusion, therefore, we can draw, is that the excessive fire, in its diffusing itself from the interior surface of the bottle through my body, finds in the exterior surface of it, an equal deficiency of natural fire, which it then entirely fills up.

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205. The charges and discharges made with naked or uncoated bottles, render still more evident the equality and reciprocal dependency of the two contrary electricities, that take place on the two opposite surfaces of the insulating body. I touch the bellied part of a naked bottle with only two of my fingers, and I charge it in that manner. I charge it afterwards, by touching it with three, then with four, then with my five fingers. I charge it again by holding it with one hand, then by embracing it with both; and each of these charges takes more time to be completed, (which is known by the electroscope taking more time to attain its greatest divergence) according as the exterior naked part of the bottle has been more amply touched; that is to say, the excessive fire does not accumulate itself on the interior surface, but correspondently to those parts of the exterior surface which are touched, and through which, therefore, an equal quantity of natural fire can escape.

206. The charges and discharges made with sparks demonstrate with still more clearness the mutual dependence of the two contrary electricities, on the two surfaces. At a little distance above the superior coating *fg hi* of the square *ABCD* (Pl. II. Fig. 8.) is suspended a metallic rod, terminated in a ball; and meanwhile the square stands insulated on the little column of glass *KL*. In such a state of things, only a very little quantity of fire flows from the ball to the square; which is, as much as can pass from the inferior surface of the square into its coating, and as much, besides, as is necessary to produce in the superior coating an excess equal to that in the Chain. Besides those small portions of fire, no other, I said, flows from the ball to the square; but, afterwards, as often as I present my finger (which I always rapidly withdraw) to the inferior coating, two sparks leap in the very same instant, the one from the said inferior coating to my finger, the other from the ball to the superior coating of the square. These sparks are always equal to each other (I perceive better their equality when, instead of my finger, I present to the inferior coating a ball of metal equal to that which hangs over the superior coating); but we must observe that the successive pairs of such sparks, are continually decreasing, and, meanwhile, the divergence

vergence of the electroscope increases. At last the sparks fail, and I find in the square a charge which is the stronger according as the ball was nearer to the square; the reason of this last circumstance is, that an electricity, though small in the superior surface of the square, is sufficient to restrain the electricity of the Chain, if the resistance which the intermediate air also opposes to the latter, be much increased by the distance. Most certainly the above experiment demonstrates that no quantity, even the least, of electric fire, can accumulate itself on one of the surfaces of an insulating body, but inasmuch as equal portions can be thrown out from the opposite surface.

207. If the ball suspended above the square, communicated with the Machine, the experiment would furnish exactly the same consequence, except with regard to the direction of the sparks, which then will be contrary to what it was in the former case: this also confirms that no portion whatsoever of fire may be taken (by communication) from the surface of a glass, but inasmuch as an equal dose is supplied on the opposite surface.

208. The discharge by sparks speaks no less eloquently. Let a ball of metal be presented to the inferior coating of an insulated square: if the latter is fully charged, no spark can leap from the inferior coating to the said ball; but if I touch, being insulated, the superior coating (I must observe to touch a strange body when I have once touched the square) two equal sparks then leap at every touching; the one from the superior coating to my finger, and the other, equal to the former, from the inferior coating to the ball.

209. I make the same experiments more expeditiously with a bottle A B (Pl. II. Fig. 5.) which I raise by the help of the insulating handle E F: no spark leaps to the hook A, from the conductor C, but as I present my other hand to the bottle, and thus enable an equal quantity of natural fire to leap to it from the exterior surface of the bottle. And thus, by presenting my finger at several times, sparks after sparks are excited, and the charge effected. The bottle being charged, I carry it to the Machine, which is animated by a contrary electricity; and I discharge it likewise by

corresponding sparks ; but these prove stronger if they are formed by the excessive electricity of the bottle, and the deficient electricity of the Machine together.

210. Besides this method of effecting charges and discharges by the means of pairs of *fellow* sparks, there is another method, similar to the former, which I call charging and discharging by alternation. Such discharges consist in making the two coatings of an insulating body alternately touch two different systems, electrified in a contrary way ; in each of these contacts sparks are produced, which are proportioned to the electricities of the two systems, directly, and also to the sums of the capacities of the two coatings ; but inversely to the sum of those sparks which have preceded them.

211. I hold by the help of an insulating handle F, the bottle A B, and rapidly move it from the Chain C to my finger I (Pl. II. Fig. 5.) which I keep at a sufficient distance, so that the sparks cannot leap, at the same time, from the conductor C to the hook A, and from the bottle B to my hand ; and at every contact between the Chain and my finger, distinct decreasing sparks are thrown, which continue for a good while, but at last become unperceivable : when this happens, I hold with one hand the belled part of the bottle, with the other I take the hook, and then I receive a stroke proportioned to the number of the sparks that have been excited.

212. I charge strongly the bottle, and holding it by the insulating handle, I alternately touch the hook and the bottle, and the consequence is, that equal sparks alternately burst out between the hook and my finger, and between my finger and the bottle : in proceeding thus with patience, these sparks will be found, first to decrease, and then totally to fail.

213. I insulate myself, and holding the bottle by the hook, I alternately touch the Chain with the said hook, and a strange body, with the belly of the bottle ; the bottle becomes charged by virtue of the alternative sparks which then take place, and are in this case very strong, decrease very fast, and a less number of them is in consequence requisite to complete the charge.

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214. Standing thus insulated, and continuing to hold the charged bottle, a man placed on the ground, touches the hook and the bottom of the bottle, alternately; and thus discharges it, by a series of sparks, which prove the stronger as their number is less.

215. I repeat these charges and discharges in the very same manner as I have done in the preceding paragraphs; only, after having insulated myself, I communicate first with one, then with two, afterwards with three men, likewise insulated; and the charges, as well as discharges, are completed by stronger, but fewer sparks, according as the number of insulated men who communicate with either the exterior or interior surface of the bottle is greater; that is, according as the capacity of the bodies that communicate with either of the coatings has been made greater.

216. These experiments may be repeated by making one or more insulated men communicate with the hook, and likewise, one or more insulated men communicate with the bottle; and it may then be observed that universally the strength of the sparks increases, and their number decreases, in proportion as the capacity of one or both the coatings of the bottle increases; inasmuch, however, as the electricity of the animated system from which the charge is drawn, will allow it.

217. This *proportionality* leaves no doubt as to the cause of those charges and discharges by alternation. Let D C (Pl. IV. Fig. 1.) represent a plate of glass; let F A E H B G represent the coatings of the plate, which may be called *a* and *b*. It must be understood that the coating F A E must communicate with the Chain; the other coating H B G is meanwhile insulated. As much fire will diffuse itself through the coating F A E, to the surface *a* of the plate, as will escape from the other surface *b* into its own coating H B G; and moreover, as much fire will flow from the Chain into the coating F A E as the capacity of this can contain; that is, the spark from the Chain to the plate, will be proportioned to the sum of both its coatings. This done, let the plate be parted from the Chain, and the coating H B G, brought to communicate with the ground; and then a spark will be thrown from the coating H B G, which will

will be proportioned to the quantity of fire in the said coating H B G, and to the quantity that will be able to get from the other coating F A E into the contiguous surface *a*; as such quantity is thereby enabled to drive another quantity equal to it from the opposed surface *b* into the ground: this second spark will be proportioned as we said, to the sum of the capacities of the two coatings. Proceeding afterwards to make the coatings alternately communicate with the Chain and the floor, the same way of reasoning will stand good; except that the faculty in the surface *a* of receiving fire, will decrease, as it will already have received more of it; and the faculty in the surface *b*, of giving fire, will decrease as it will already have given more; therefore the successive sparks will be found to decrease proportionally; and as, after a given number of sparks, the surface *a* will have received more fire, and the surface *b* will have given more, (proportionally to the greater sum of the capacities of the coatings,) so a less number of sparks will now be required to complete the charge.

218. Analogous observations may be made on the discharge of the plate. Let the plate D C be charged and insulated. When I touch from the floor the coating F A E, I receive the excessive fire accumulated in it, and draw, besides, from the surface *a* as much excessive fire as the coating H B G can give to the other surface *b*, electrified by deficiency. If I afterwards touch the coating H B G, I give to it as much fire as can adhere to the deficient surface *b* (which is equal to the quantity that may diffuse itself from the abounding surface *a* into its own coating) and moreover, as much as is necessary to fill up the deficiency in the coating H B G; and if the two surfaces are successively and alternately touched, the same effects will obtain, except that the sparks will successively decrease as the electricity remaining in the two opposed surfaces will decrease; because when, after a given number of sparks, I touch the coating F A E, a less portion of natural fire must diffuse itself from the opposed coating H B G into the contiguous surface *b*, there to acquire a deficiency of an equal density with that of the surface *b* already diminished, &c. And as the decrease of the deficiency in *b*, and of the excess in *a*, is accelerated proportionably to the sum of the capacities of the

the coatings; so the number of the sparks necessary to complete the charge, will diminish proportionally to the sum of the said capacities. It is an agreeable spectacle to see a bottle suspended to the Chain, when both (the Chain and the bottle) are supplied, each with its own electroscope. While I touch the Chain, the threads of its electroscope fall down, and those of the electroscope of the bottle diverge, being become electrified by deficiency: if I touch the bottle, the threads of its electroscope fall likewise, and those of the Chain diverge, having again acquired an electricity by excess: the sparks and divergences thus continually keep decreasing, and a less number of sparks becomes required to complete the charge, as the capacity of the Chain (with which the interior surface of the bottle communicates) and the exterior coating of the bottle are greater: so that the sparks increase in force as they diminish in point of numbers.

219. Therefore, this same proportionality, while it discovers the causes of the charges and discharges by alternation, resolves a seemingly great objection against the theory. It is this: if an electricity is produced and destroyed by turns, in each of the surfaces of the insulating body, though the other surface remains insulated, it seems that such electricity is produced or destroyed, though no correspondent contrary electricity is produced or destroyed at the same time, in the opposed surface, as the theory should require. But an analysis of the fact, grounded on the consideration of the aforesaid proportionality, proves that the electricity which is thus produced or destroyed by those alternative contacts, is in this case also governed by the contrary electricity that may be produced or destroyed at the same time on the opposed surface, proportionally to the capacity of the coatings. I suspend a bottle to the Chain by its hook; and taking care that it be exactly insulated, I excite the electricity in the Chain. As soon as the two threads annexed to the exterior surface begin to diverge, I touch the Chain, and there remains no electricity either in the Chain, or in any part of the bottle. The electric fire of the Chain was accumulated on the interior surface of the bottle in as great a quantity as it was able to repel other fire from the exterior surface,

surface, into the coating and the threads, which were as a part of the bottle; and all these were in reality electric by excess. Now, when I touch the Chain, I take the excessive fire from the Chain and the interior surface of the bottle; and correspondently, the natural fire which was kept out of the exterior surface of the glass, and harboured in the coating and threads, falls back into it.

220. Moreover, this same proportionality shews to us that it is not absolutely necessary, in order to discharge a bottle, to complete a communication between the two surfaces; since in touching them alternately, the discharge is completed by alternative sparks; the number of which is less, as the capacity of the coatings is greater. Whence it necessarily follows that if, while one coating communicates with the ground, the other is also brought to have a relatively infinite capacity, the discharge will be completed, though the latter coating does not communicate either with the other, or with the ground. In fact, I have many times, when my long deferent threads with which I use to try the atmospheric electricity, have been animated with a continued, and vehement, though not fulminating electricity. I have, I say, often taken a pleasure to touch the said threads (using always the necessary caution) with the surface of a charged bottle, which surface was replenished with an electricity contrary to that of the threads; and in a very short time the bottle was discharged, and even charged in a contrary manner. If as many men are insulated as will equal the capacity of the bottle, and all communicate together; when I present to them the hook of a charged bottle, I impart to them the half part of its excessive fire, and, in the same time, the ground will supply the exterior surface with the half part of that fire of which it has been deprived. If the capacity of the insulated men is a thousand times greater than the capacity of the bottle, only one thousandth part of the charge will remain in it. It is true that a discharge thus made by forming a communication between the opposed surfaces of the bottle, and two separated systems, will be less violent than if it were effected by forming the communication between the two opposed surfaces, just as a spark by excess and deficiency together, will be less violent than a spark produced by an excess alone, or by a deficiency alone.

221. Lastly,

221. Lastly, it follows as a necessary consequence of all these experiments, that each of the contrary electricities that may be introduced on the two opposed surfaces of an *insulating* body, surpasses by far any electricity that may be introduced into a *deferent* body, even of a much larger dimension:—thus a hundred sparks given to a coating, will nevertheless introduce into the plate, itself, but a small charge. Let us add here an experiment which may be called a charge *by dimidiated alternation*: I insulate myself between the Chain, and a bottle which communicates with the ground; and I draw successive sparks from the Chain, to which I present, at different times, the hook of the bottle: after a hundred such sparks the bottle is found to be but imperfectly charged.

222. The same is still better proved by discharges. Standing upon the ground and holding the bottom of a charged bottle, I alternately present the hook of it to the hand of an insulated man, and touch the man, after removing the bottle: after a hundred such alternative operations, the bottle has electricity enough left to give me a stroke.

223. We shall see in its proper place, that the great capacity of insulating bodies arises from the combination of the two electricities that take place on both their surfaces. If from the excessive thickness of these bodies, or in general, a want of communication between their opposed surfaces, such combination is prevented, no greater quantity of fire can be accumulated on, or taken from, them, by the mere help of a communication with the Chain, than what could be added to, or taken from, deferent bodies. We shall see besides, that the capacity of deferent bodies is no more than that of their surfaces. Let us now proceed to other simple charges and discharges, made in different ways.

224. Among them are the charges and discharges made by the means of *brushes* of water; which serve still better to confirm the theory. From the conductor C (Pl. IV. Fig. 2.) a rod of brass is suspended, which dips in the water of the basin K I, placed on a stool of cement M L; I hold by the insulating handle A B the bottle C D, into which one arm of the siphon P A F, is also dipped, and the water issues from the other arm, and falls into the basin K I; meanwhile I touch with my finger E the outward coat-

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ing of the bottle. As soon as the electricity of the conductor is excited, the *brush* of water P spreads itself, and the bottle becomes charged.

225. I repeat the experiment, but without touching the exterior surface of the bottle; when I find the brush of water is no more; the bottle takes no charge; and when I try it, as I did just now, I receive no stroke.

226. I repeat once more the experiment, but when the brush issues I touch and leave the bottle alternately; the consequence is, that the brush does not spread itself but correspondently to the time in which I touch the bottle; and the bottle gives no stroke but in proportion with the time during which it has been touched.

227. Let now the bottle be supposed charged, let the basin lie on the ground, and the communication with the Chain cease; if I let the water issue without touching the outside of the bottle, the brush does not spread, the bottle takes no charge. If I take away the siphon, and substitute the hook, I receive a stroke. If I constantly touch the bottle, it loses its charge and I have no stroke. If I touch it and leave it by turns, it is discharged proportionally to the time during which I have touched it; and gives a stroke which is proportioned to the time during which it has remained untouched.

228. Whence it generally results that these laws of the brush and of the stroke, make it visible and audible, that an electricity cannot be raised on a surface of an insulating body, but inasmuch as a contrary electricity is raised, at the same time, on the opposed surface; and that such an electricity cannot be taken away but inasmuch as the contrary electricity is taken from the other surface.

229. Charges and discharges by motions, succeed more easily, and prove the same things. I hold the bottom of a bottle, and present its hook to a pendulum B (Pl. V. Fig. 4.) of a great electric capacity (it is made with a sheet of gilded paper, folded in the shape of a cylinder) which hangs by a long silk thread, near either the Chain, or the Machine D; this pendulum, as it vibrates, carries sparks from the conductor to the hook, and the bottle becomes charged. I afterwards raise the bottle by its hook, and present it

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to the pendulum, and thus make the latter carry to the outside of the bottle, the fire that had been driven from it by the fire introduced through the hook, into the inside of it: and thus the bottle is discharged.

230. I can likewise discharge the bottle by continuing to hold it by its bottom, and keeping the pendulum in the middle between my other hand and the hook of the bottle: in this case, the pendulum takes the excessive fire from the inside of the bottle, and, through my body, carries it to the exterior surface of it; this will equally obtain, whether I am insulated or not.

231. In charges, as well as discharges, the pendulum vibrates at first more strongly, because it transports more fire in proportion as it has yet transported less, if a charge is producing; or in proportion as it has more to transport, if a discharge is effected; and the oscillations are thus continually decreasing: with this observation, however, that the oscillations in a discharge, as they are produced by positive and negative electricities together, are quicker than those of a charge.

232. If it were intended more minutely to examine by the means of motions of a *simple pression*, the manner in which charges and discharges are effected, the truth of the theory would appear with still more clearness. I. I fix an electroscope to the exterior coating of a bottle; I suspend this bottle to the Chain supplied with its own electroscope, and I constantly touch the bottom of the bottle: when I find, that the electroscope of the Chain gradually increases its divergency, as the charge of the bottle is increasing; but yet, that the electroscope of the bottle remains without motion. The reason is this;—in proportion as the excessive fire accumulates itself in a greater quantity in the inside of the bottle, and has driven a greater quantity from the outside, it meets with a continually greater resistance from accumulating itself farther, and driving away more fire from the outside; consequently, it is kept back, and is accumulated in a greater quantity in the Chain. But as, at the same time, all the fire that is driven from the outside of the bottle, can flow through my body into the ground, it consequently cannot produce any excess in the exterior coating: nor, for the same reason, can any part of the deficiency produced and

maintained in the exterior surface of the glass, stop and manifest itself in the said exterior coating.

233. II. I repeat the experiment with only this difference, that I now and then take my hand from the bottle; and I observe, at every time, that the electroscope of the Chain immediately attains its greatest divergency; and also, that the electroscope of the outside of the bottle, acquires divergence proportioned to the charge already produced, inversely. That is to say, as the outside of the bottle becomes insulated, no fire can escape from it; no fire, therefore, can flow to the interior surface: consequently, it is forced to remain and accumulate itself in the Chain, and the electroscope immediately diverges to the utmost. Moreover, in proportion as the charge will be advanced, a less quantity of fire will be driven from the exterior surface of the glass into both its coating and the electroscope annexed to it; so that a less divergence will consequently take place in the latter; but, when at last the charge is completed, no divergence will be perceived, though I take my hand from the bottle.

234. If, after completing the charge, we pass to the discharge of the bottle, suspended for instance to the Chain, we shall see that when the discharge is effected with a conducting bow which is not insulated, no divergence will remain either in the electroscope of the Chain, or in that of the bottle; unless perhaps some arises afterwards from a remainder of the *overflowing* electricity, as I will mention hereafter. But then if I effect the discharge with an insulated bow, some divergence will remain in the electroscope of the Chain, and an equal one will arise in the electroscope of the bottle. The reason is, that when the contrary electricities of the two surfaces of the bottle will have destroyed and compensated each other, the excessive electricity of the Chain and hook will distribute itself to an equal density into the insulated conducting bow, and the two coatings of the bottle. But, in the charge of *independency*, which I shall describe in the paragraph 240, the conducting bow will leave no remainder of electricity; and the reason will be that, in that case, no fire will be accumulated on one surface of the bottle and its coating, but what will have been entirely extracted from the opposed surface and coating.

235. We

235. We might now discuss the case of a discharge of the bottle, by alternation, but we have already considered it. To be short, when a coating is touched, the electroscope annexed to it falls, and that annexed to the other surface rises, though, we must observe, in proportion to the remainder of the charge.

236. In order to examine the charges and discharges produced by such motions as result from alternate attractions and repulsions, I usually present to the coating of the bottle, a small piece of tinfoil †, hanging by a thin silk thread. And first, if, as in the parag. 232. I present such pendulum of tinfoil to the exterior coating, it receives no motion; because, as we have seen, there is no electricity in the coating, since it communicates with the ground; but if I present the same to the Chain, which is, in a manner, a prolongation of the interior coating, then, if the silk thread is somewhat damp, the tinfoil is immediately attracted; or, if it be very dry, I only need touch it with my finger *, and the tinfoil flies to the Chain; from which likewise receiving an electricity by excess, it is instantly repelled. In this state of things, if I present that same piece of tinfoil to the exterior coating, it is attracted by it, as it would be by any other part of the ground: so that it diffuses into the coating, or into the floor with which the coating communicates through my body, all the excess it has received from the Chain, or rather from the interior coating. If I present it again to the Chain, and if it be very dry, it will not be either attracted or repelled, till it has been touched.

237. II, in the case of the No. 233. the piece of tinfoil, when repelled by the Chain, is also repelled by the exterior coating; which shows that this exterior coating, really becomes electric by excess in consequence of the natural fire of the outside of the glass which stops in it, while I cease to touch it; though (which

* Besides the general law, that bodies similarly electrified mutually repel each other, and differently electrified bodies mutually attract each other, I must here take it for granted (which I shall hereafter prove) that a body not electrified cannot be moved by one that is electrified, but inasmuch as the latter will be able by the means of its electric atmosphere to introduce into the former an electricity contrary to its own: and, in order that the Chain may introduce a contrary electricity into the piece of tinfoil abovementioned, it must be enabled to drive away the natural fire in it, either through the damp thread annexed to it, or through any other body in contact with it.

† Or German leaf-brass.

is extremely conformable to all that has been said) this last repulsion produced by the coating, is less strong in proportion as a greater quantity of charge has been introduced, while I ceased to touch it, as has been said before.

238. In the case of the No. 234, that is, in the case of a complete discharge, there is nothing farther to be observed with regard to the piece of tinfoil, except perhaps that when it was in a state of attraction or repulsion, it would have been reduced to its natural state, if a communication had been formed between the two opposed surfaces of the glass, by the means of a deferent body communicating with the ground; or the same piece of tinfoil would have been brought into a state of repulsion, proportioned to the quantity of electricity remaining in the two coatings, (234) if the communication had been only formed with an insulated body.

239. We are now to examine the case of a bottle that is charged, and has its two surfaces exactly insulated. Let us suppose, first, the case of a bottle suspended to the Chain. I. If the piece of tinfoil be presented, first of all, to the bottle, and even is made to touch it, yet it will not move, if the outward coating of the bottle has been brought to have no electricity in it. II. But if the same be presented, first, to the Chain, I shall no sooner have touched it, but it will be drawn, and then repelled; because it will find in the Chain an excess correspondent to that in the inside of the bottle. III. If the piece of tinfoil be immediately presented to the exterior coating it will then be drawn and repelled by it; because it will have received an excess from the Chain, and will now find a corresponding deficiency in the exterior coating. The reason is this: the Chain, which is a prolongation of the interior coating, required to have in it a certain excess, in order to be enabled to maintain, jointly with the excess on the inward surface of the glass, the deficiency in the exterior surface of it; so that the exterior coating then retained its natural quantity of fire; now, in consequence of the chain having drawn the piece of tinfoil, some portion of its excess has been taken away, it will of course be no longer able to maintain in the outward surface of the glass, as great a deficiency as it did at first; some part of the natural fire in the outward coating will then pass into the outward surface of the
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the glass under it; and consequently that coating will contract an electricity by deficiency. IV. Lastly, if, the bottle remaining exactly insulated, as above, I make the piece of tinfoil vibrate between my finger and one of the coatings, the pendulum will constantly draw from that coating, a quantity of electricity equal to such contrary electricity as may flow from the opposed surface into its own coating: then the pendulum will cease to vibrate, and must be presented to the other surface, where it will begin again to play, and at last likewise cease; and in short a discharge will be obtained by oscillations that alternately cease and begin again: they are reproduced by turns, in the same manner as electric signs are reproduced by turns between the Machine and the ground; or between the ground and the Chain, when both systems are insulated.

240. Every body evidently sees how completely conformable these motions are to the theory, and I may easily exempt myself from giving any farther explanation: I shall therefore conclude this chapter with a more explicit account of a fine experiment of Dr. Franklin, which I have mentioned before; I call it a charge of *independency*. I hold by its bottom B the bottle A B annexed to the hook of the conductor C. (Pl. III. Fig. 10.) The globe or cylinder *a b c d*, is rubbed by a cushion insulated by the support of glass V, which is fixed in a hole of the beam Q (Pl. I. Fig. 1.) then I find the bottle does not become charged; nor is the globe able to carry into it any other fire than that contained in the cushion itself. But if I join the outside of the bottle to the cushion, by the means of a wire of metal B R, then, notwithstanding that I do not touch the bottle, it becomes charged, and even more strongly than usual: the reason is, because the globe is supplied with the fire which it accumulates in the inward surface of the glass from the very outward surface of it; and the concurrence, in this case, of the extracting with the accumulating force, increases the intensity of the charge. As Charges are usually effected by making one of the surfaces communicate with the ground, in this case one of the two forces is, as it were, lost in the infinite capacity of it, and Charges consequently only correspond to the other force.

241. Certainly, I do not think that any other experiment can so well, and so simply at the same time, demonstrate both the equality and contrariety of the two electricities, as does that charge of independency; especially if such charge be compared with the discharge that I call likewise of *independency*; which is effected by a Man standing insulated, or which is still better, by the means of an insulated bow.

C H A P. III.

In which the theory is confirmed by a combination of two or more charges and discharges.

242. **I** Hold with one of my hands the bottom of the bottle A B, and with the other, the bottom of the bottle *a b* (Pl. II. Fig. 6.) and I keep their hooks in contact with the Chain: in which case, the electroscope annexed to the one of them, attains but slowly its greatest divergence. When it has attained it, I remove the two bottles at once, and place them on an insulated bench; then I immediately take again the bottle A B, and, with my other hand, touch the hook of it; I do the same with the bottle *a b*; each of them gives me a stroke proportioned (every other circumstance being equal) to its own capacity. This is because the electric fire of the Chain tends equally to accumulate itself in all the equal parts of the coated inward surfaces, and to drive corresponding quantities of natural fire from the other outward surfaces.

243. I charge again the two bottles jointly, and in the same manner; and after removing them from the Chain, I bring their hooks into a mutual contact: but no light is produced, no spark flies, though the capacities of the two bottles be ever so different: this is because the same stream of fire being distributed into the two bottles, proportionally to the capacities, retains in them an equal density; consequently, the two excesses reciprocally and equally repel each other.

244. I say that the two charges jointly of the two bottles are proportioned to their two capacities together; but this is when

other circumstances are equal. I make use of two bottles of the same size, and the coatings of which are equal; only the bottle *ab* is thicker than the bottle *AB*: and the latter (the thinnest) strikes me more violently than the other. The reason is this; the excessive fire that flows into the bottle *ab* finds more difficulty to drive through its substance the natural fire from the opposed surface: now, as the excessive fire cannot accumulate itself on the interior surface of the bottle, but in proportion to the quantity of natural fire which it can drive from the outside of it, it follows that the intensity of the charge must be less when the glass is thicker.

245. I have hitherto considered the cases of discharges effected subsequently to the charges; now I proceed to examine the cases of charges and discharges effected at once. Having jointly charged the two bottles *AB*, *ab*, I place them on an insulated stool, then I take them both in my hands, by their long uncoated necks, and incline them so as to make the hook *a* touch the bottom of the bottle *B*; and in the instant the communication is effected, a middling spark is thrown out. Then I bring the hook *A* into contact with the bottom of the bottle *b*, and at that instant the discharge is completed. That is to say, when at first the bottle *B* touches the hook *a*, a quantity of excessive fire passes from *a* into *B*, equal to that which it can drive from *A* into the air, or such bodies as stand at a little distance, for instance, my hands, and to the natural fire that may be drawn into *b*, from the air, or bodies placed near, by the joint efficacy of the deficiency in *B*, and excess in *a*. But when afterwards the hook *A* and the bottle *b* are likewise brought into contact, all the excess in *A* throws itself out to fill up the deficiency in *b*; because reciprocally all the excess in *a* can then run to fill up the deficiency in *B*: so that, examining afterwards the two bottles I receive no stroke (except perhaps from some remain of charge, of which I shall speak in its place) if the bottles were of an equal capacity.

246. The reason is this, if the capacities of the bottles are unequal, such bottles jointly and equally charged, and then made to communicate together as before (the hook of the one with the bottom of the other) are found, after such communication, to

have contrary electricities on their homologous surfaces; and these electricities are proportioned to the difference of their capacities. Let, for instance, the capacity of the bottle A B, be to the capacity of the bottle *a b*, as 20 to 5: if I make the hook of A communicate with the bottom of *a b*, and likewise the hook of *a*, with the bottom of A B, $\frac{5}{20}$ of the excess in A will pass into *b*, and fill up its whole deficiency; and the whole excess in *a* will pass into B, and fill $\frac{5}{20}$ of its deficiency. In consequence of this discharge of the fourth part of the excess in the bottle A B, the other *a b*, will be brought to have the natural quantity of fire, on both its opposite surfaces; that is to say, will be thoroughly discharged. But fifteen parts of excess, we must observe, will remain in A, and fifteen parts of deficiency in B: these remaining parts will also distribute themselves into the two bottles, in proportion to their capacities; that is to say, $\frac{3}{5}$ of the excess in A will pass into *b*, and $\frac{12}{5}$ remain in A; and a quantity of natural fire will also from *a*, pass into B, and supply $\frac{3}{5}$ parts of its deficiency, so that only $\frac{12}{5}$ will remain. All this may be proved in different ways. I. By severally discharging the two bottles: both the light and crack of the spark, as well as the strength of the shock, will be found to answer to their respective capacities. II. By making the two bottles again communicate, the hook of the one with the hook of the other, and the bottom of the one with that of the other: a second discharge will in this case be obtained, which will also be found to be proportioned to the difference of the capacities of the bottles. I have, several times, amused myself to charge, jointly, a great jar of crystal, which contains within the capacity of its lining, sixteen pints of water, and a small lined jar which only contains the fourth part of a pint: then, holding the latter with an insulating handle, I touched at once, with its hook and bottom, the bottom and hook of the large jar, then the hook and bottom, and so on, a number of times, alternately; when I had no small pleasure to observe how the sparks from the alternate discharges went gradually decreasing; and how in each of these decreasing discharges, two short threads annexed to the small jar, at once fell, and then gradually resumed a less divergence than before; while two threads annexed

annexed to the larger jar, without falling, only lost a little of their former divergence.

247. Now, these discharges thus jointly effected a number of times, by alternately bringing together the contrary surfaces of two charged insulating bodies, are those discharges which I call of *simple distribution*. Let us now subjoin an experiment which certainly will confirm the theory laid down in this work with regard to the cause of charges, and also serve to illustrate the manner in which complicated charges and discharges, which are the subject of this chapter, are effected. I charge a bottle A B, from the Chain, and holding it by its bottom, I present its hook to the hook of a similar bottle *a b*, which I hold in my other hand. At that instant, a spark flies between the two hooks, and I receive a shock: half part of the excess in the charged bottle A B (I suppose the two bottles to be of equal capacities) runs into the bottle *a b*, which is without charge; and an equal quantity of natural fire runs from the outward bottom of the latter, through my own body, to supply half part of the deficiency in the outward bottom of A B. In fact, if I afterwards try separately to discharge those two bottles in the usual way, I receive two different shocks, which I find to be equal to the former one, and, of course, equal to each other.

248. It is a difficult matter to find thus two bottles of equal electric capacities. I try them by charging them, jointly: if their electricities exactly destroy each other, when I make their contrary surfaces communicate together, I call them *equivalent bottles*.

249. I take a series of such *equivalent* bottles, A, B, C, D; I charge the bottle A, and grasping in my hand the bottle B, which is without charge, I bring their hooks together: half part of the charge in A distributes itself into the bottle B. I then lay by the bottle A; take up the bottle C, and make its hook touch the hook of the bottle B; when the half of the charge just introduced in B, distributes itself into C. I lay by the bottle B, take up D, and make its hook communicate with that of the bottle C; when again, the half of the charge introduced in C, distributes itself into D. I then resume each of the four bottles successively, and discharge them in the usual way, through my

own substance; when, by estimating as far as I am able, the intensity of the charge, from the strength of the spark and stroke, I find I must say, that the charge remaining in A, is double to that remaining in B; that remaining in B, double to that remaining in C; and that in C, equal to that in D, the charge of which has not been farther divided.

250. But let us return to charges made jointly; and since we have examined *similar joint* charges, that is, those by which similar electricities are raised on the homologous surfaces of the two bottles; now let us consider the contrary charges, that is, those by which contrary electricities are raised on the homologous surfaces, by applying them either to the same, or to different systems. I raise the bottle *a b*, by its hook, and keep its bottom in contact with the chain, while, with my other hand, I hold the bottom of the bottle A B, and touch the chain with the hook annexed to that bottle. This done, I lay the two charged bottles on the insulating stool (Pl. II. Fig. 6); I then take them by their necks, and inclining their hooks, I make the hook of the one communicate with the bottom of the other, when I find that no spark appears. Having again raised the bottles, I touch hook with hook, bottom with bottom, and they are discharged: besides, if while I approach the hooks to each other, I hold the bottles by their bottoms, I receive a vehement stroke. The reason is this, all the excess raised on the outside of the bottle *a b*, flies through my body, to fill the deficiency raised on the outside of the bottle A B: mean while all the excess raised within the bottle A B, throws itself into the hook of the bottle *a b*, and fills the deficiency likewise raised on its inside.

251. Let the Machine and Chain be insulated; I hold the bottle *a b*, and present its hook to the Machine, while, with my other hand, I present the hook of the other bottle to the Chain. Then I remove them at once, and present the hooks to each other; when I receive a stroke, and the bottles are discharged. This is to be explained thus: the Machine has extracted the natural fire from the inside of *a b*; and, through the Chain, has introduced it into the inside of A B: therefore the natural fire has been driven from the
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outside of A B, and a quantity of excessive fire has been accumulated on the outside of *a b*.

252. We are now to treat of the *consecutive joint charges*. I suspend a bottle to the Chain; it being thus insulated, takes no charge. I touch its bottom with the hook of another bottle, of which I grasp the bottom; both become charged; and, if I try them, give me a stroke. If I touch the hooks with each other, though I also touch the bottom of the bottle, there is not the least beginning of a charge. If I grasp the bottom of *a b*, (Pl. II. Fig. 6.) and raise A B by its hook, so that its bottom be touched by the hook of *a b*, both become charged. The excessive fire which accumulates itself from the Chain, on the inside of the bottle annexed to it, drives an equal quantity of natural fire from the outside of the same bottle, into the inside of the other; and this fire, in its turn, drives from the outside of the latter bottle through my body, an equal quantity of natural fire.

253. This experiment may be made with three, four, or more bottles that successively hang from one another by their hooks: but as the number of these bottles increase, the intensity of the charges will decrease: the reason is, because the total thickness of the glass, proportionally to which the charges must decrease, will then be increased.

254. It sometimes happens that when I successively discharge the said bottles, one of those in the middle will give a greater stroke than that before it; this arises from two causes chiefly, viz. from the more exact insulation of one of the successive bottles; and from the less exact discharge of that which is before it. If any portion of the excess that is accumulating in the inside of the preceding bottle, flows from the inward into the outward coating of it, by means of any communication arising from dampness or moisture, no other charge will take place within such a bottle, but what may arise from such portion of excess as cannot escape through the said different particles: and in the meanwhile, if the following bottle has both its surfaces better separated from each other, in consequence of a more exact insulation, the charge it will receive will be equal to the two portions that will contribute to form it; that is, both to the portion of excess that has
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thus externally escaped, and to such portion of fire as remained accumulated within this same preceding bottle.

255. Moreover, the charges of the two successive bottles may be equal to each other, and yet the discharges appear very unequal. If, for instance, the hook of the preceding bottle has any part of it too rough, or too pointed, the discharge will be effected in a divided manner, and consequently give a less stroke. On the other hand, if the hook of the successive bottle has in it any portion of moisture, of which the hook of the preceding one we may suppose was free; this will open a wider way to the discharge of such bottle, as I will shew in its proper place: whence it will happen, that the spark leaping more instantaneously, will strike with a greater force. I confess that the reason I give here of such unequal discharges arising from equal charges, is not of itself very obvious; but this is because the instant by which one of the discharges is, as I say, made longer, and consequently weaker, than the others, is so short, that our senses cannot perceive it, nor find the difference it produces between the times in which the above mentioned discharges are severally performed.

256. I shall observe here, by the by, I. That *successive* charges of bottles may be effected in such manner as to have the homologous electricities take place on *contrary* surfaces of them. If I hold, for instance, a bottle by its hook, and bring its bottom to touch the bottom of another that hangs from the chain by its hook, the latter will be charged by excess in its inside, while the former is so on its outside, &c. II. Any charges whatsoever made by the means of the Chain, succeed as well, though in a contrary way, as those made by the means of the Machine: that is, there results no other difference besides that of the electricities produced upon the same surfaces, being contrary. III. All the experiments which I have described as made with bottles, will equally succeed with glasses of any other shapes; such as common vessels, plates, tubes, &c. provided they are thin enough, properly coated, and made use of consistently with their form. All these observations are too obvious to be more explicitly insisted upon:—I shall only observe, that the general mutual agreement between all the above experiments, throws a most complete light on the subject, and makes the certainty of the theory perfectly evident.

257. How-

257. However, in some cases, a certain shape in the glass is more convenient than another. For instance, when I examine how far the charges are lessened by the thickness of the glass, I prefer plates of glass to bottles, because the thickness of the former is more uniform, the difference between them is more easily estimated, and their electric *capacities* may be made more completely equal to one another: they will not, it is true, be so easily managed, but the exactness will be greater, and the observations that will result from the experiment will prove more simple and easy. Thus, I select two plates $ABCD$, $abcd$ (Pl. VII. fig. 10.) the one is thicker than the other in a given proportion, but their metallic coatings are completely equal. I place one of the plates on one of my hands, and the other plate on my other hand; I then present them to two rods of brass that proceed from the Chain; and, when the electroscope informs me that the charges are completed, I remove both plates in one and the same instant (which is easy, from the use we have of making similar motions with both hands at once); this done, another person immediately takes both plates by their angles, and keeps them in an horizontal situation, at a proper distance from each other; then I take with handles of sealing-wax I , i . the two conducting bows, which are bent, the one in the shape $\frown$, the other in the shape $\smile$, and I place them so, that each communicates with the different surface of each of the two plates; when I find the effect to be that the excess in E , and the deficiency in H , and correspondently, the deficiency in h , and the excess in e destroy each other, as far as they are equal to each other. Besides, the overplus of excess in E distributes itself; that portion which remains in E , is to that portion which passes into H , as the thickness of $ABCD$, is to the thickness of $abcd$: and the overplus of the deficiency in h is communicated to e in the same proportion: so that I find the electricities in $ABCD$, inverted, and they are to the electricities that remain in $abcd$, as the thickness of $abcd$, to the thickness of $ABCD$.

C H A P. IV.

In which the Theory laid down in this Work is confirmed, by charges and discharges of glasses, to which the vacuum serves as a coating.

258. **I**F an insulating body, though capable of itself of taking a charge, be placed in a space exactly emptied of air, it cannot be charged: the reason is, that any quantity of fire that will flow to the one of the surfaces of such body, will, in consequence of the exact vacuum, diffuse itself to the other surface; therefore, there will be no excess in the one, no deficiency in the other.

259. But as the vacuum in the air-pump never is brought to perfection, then it follows that the insulating body placed in such vacuum, will receive some beginning of a charge; which will be proportioned to the imperfection of the said vacuum; this can be perceived in that experiment which I formerly mentioned, in the parag. 272, of the *Artificial Electricity*. Let the fig. 13. of Plate IV. represent the necessary apparatus, which consists of a glass bell emptied of air, with the half of a ball, or an hemisphere, of very thin glass, inverted on the basin. The electric ray BC flows from the Chain, through the rod AB, into the top of the said hemisphere: the ray seems, at first, to stop in C; and, correspondent to it, a faint light appears on the top of the pipe of metal which rises from the basin of the air-pump; but soon after, the electric ray diffuses itself from C all over the surface of the hemisphere, and runs along it (in D) to the basin under it: nor does this ray CD, constantly keep to the same track, but keeps turning about on the surface of the hemisphere of glass. Now, both this stopping of the ray in C, and its turning afterwards around the hemisphere while flowing along it, show that a small portion of excess is accumulated on the surface of the said hemisphere, both on that place to which the ray flows at first, and on those about which it winds. The light on the top of the pipe of metal, indicates that a small portion of fire has been driven from the inward surface

surface of the hemisphere, and in short, that there is a beginning of a charge, at least of a transitory one, proportioned to the imperfect *deference* of the vacuum. In fact, according as I let in any additional portion of air, the ray BC, not only becomes more visible, less continuous, and narrower; but it stops longer in C; leaps in a greater quantity to such other points of the hemisphere as are situated obliquely to the rod AB; subdivides itself into a greater number of rays as it flows along the surface of the hemisphere; and growing gradually fainter, successively vanishes. Correspondently to this, more vivid rays of light throw themselves, from the inward surface of the hemisphere, into the top of the pipe of metal, and even into the basin, where the latter is in contact with the interior edge of the hemisphere.

260. A similar explanation may be given of those charges which are raised on a glass bell, within which the air has been dilated. The fire which from the blunted rod AB (Pl. IV. fig. 14.) directs itself at first to the basin, bends afterwards its course towards the finger presented by the hand C, because as the distance of it is less than that of the basin, the dilated air interposed between it and the point B, opposes less resistance to the fire, than the air equally dilated which lies between the said point B and the basin. If the whole palm of the hand be laterally applied to the glass bell, a large quantity of fire will run to the corresponding part of the inward surface of the latter; which fire will drive an equal quantity from the outward surface contiguous to the hand: and thus a small charge may be obtained, which would give a stroke to a man who, keeping his hand C applied to the bell, would carry his other hand to the hook A, or to the Chain with which the same communicates. I say, a small charge, because the fire that will run to that part of the inward surface which corresponds to the hand, will stop there only in proportion with the very small resistance which the dilated air will oppose against its reaching the basin. If the air-pump be insulated, the charge can then be greater, because when the pump will be *saturate*d, all the fire will be determined towards the hand. Care however must be taken that the glass bell be exactly polished and dry,

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and of a glass sufficiently thin ; that the extremity B of the rod be not too far from the place to which the hand is applied ; and that the said hand be applied to a place sufficiently distant from both the metallic armature of the neck of the glass bell, and the basin on which the same is placed.

261. An easier and stronger charge will be obtained by applying the animated system, not to the inside of the glass, to which the vacuum corresponds, but to the outside, which is surrounded by the air of the atmosphere. After the example of the Abbé Nollet, I fixed with cement the neck B of a bottle of thin glass, within the large neck E of a glass bell (Pl. III. fig. 9.), so that the bottom of the bottle remained within the capacity of the glass bell. Having formed the vacuum, I made the hook A communicate with the Chain, and the bottle became very strongly charged : so that, a person who touched with one hand the basin F G of the air pump, and carried the other hand to the hook, received as strong a shock as could be effected with any bottle of the same capacity. The Abbé alledged that experiment to prove that the electrical fire can, as it were, ooze through glass, and escape through its substance. He pretended that while the bottle was charging, the rays of fire never ceased to flow from the inside of the same to the basin, across the dilated air ; but in the third volume of his letters, he has been candid enough to give up this opinion. The fact, therefore, is, as I wrote in the letter added to my book of *Artificial Electricity**, that in charging the bottle, a kind of lucid semicircles are seen flying about the bottom of it, which unite together near the lowest part, into one ray that directly proceeds to the basin of the air-pump. But these semicircles and ray, after some windings, grow languid, and do not appear but in an interrupted manner, and in those instants especially when some more strength being used in the friction, the electricity of the glass is increased : when the friction is made uniformly, they disappear intirely. Care must be taken, therefore, to keep the friction uniform ; and above all, that, while the

* A former work of the author's.

charge is forming, the electricity of the Chain be not diminished either by the vicinity of a strange body, or any other cause. The reason is, that should any such thing happen, the fire accumulated within the bottle would flow back into the Chain, and an equal quantity would run through the vacuum of the glass-bell, to the bottom of the bottle; so that the charge, instead of moving forwards, would move backwards, and the rays which would then be mistaken for a continuation of the same charge, would, in fact, be the beginning of a new one. The said Abbé used, in this experiment, to cover all the basin of the air-pump with a very thick layer of sealing-wax, which, as every body sees, must stop, in consequence of its repressing quality and thickness, the electric fire which sprung out of the bottle; so that, there is no wonder that, not being able to proceed farther, this fire continued to vibrate and produce a light.

262. The light from the discharge of the above bottle, is indeed very different from the light produced by its charge. The light from the former, at the instant it is effected, forms around the bottle a kind of splendid coat, which, if attentively considered, seems to be composed of small rays, converging to the surface of the bottle. Indeed the different forms assumed by the electric fire, in its springing out of the surface of the bottle, and in its getting back into it, might alone afford proofs of the difference of its directions, in these two cases.

263. That discharge which is made by touching at once the basin FG, and the hook A, is extremely simple; but if, instead of effecting it in that manner, the palm of the hand be applied to the glass bell itself, nearer to the bottom of the bottle, the discharge will then be complicated with a transitory charge of the glass bell. This charge will arise from a great portion of fire flying from the inward surface of the glass bell, to the deficient bottom of the bottle. That part from which the fire will fly, will be the same which corresponds to the hand, as the latter can supply an equal excess on the outside of the bell, and meanwhile the other hand diffuses away the excess accumulated in the inside of the bottle. The light which, in that kind of discharge, abundantly sparkles within the glass bell, between the bottle

and the place where the hand lies, is a sign clear enough of a transitory charge of the glass bell; and the strong shocks I have had from it, have moreover thoroughly convinced me of the truth of the observation. But such charge never can be, as I say, but a transitory one, because both the deficiency introduced in the inward surface of the glass bell, and the excess introduced on its outward surface, can be diffused into the ground; the former through the dilated air, the latter through my body.

264. As the discharge of the bell may also be obtained by simply touching the basin, it is not necessarily *complicated*; therefore, the charges and discharges made by the means of a vacuum which I have hitherto explained, may be called both *simple* and *single* ones; but there are other discharges also made with a vacuum, that are *double* or even *treble*, and in which from one charge or discharge, two or three others follow. I shall rank first among these successive charges and discharges, the *double* ones effected by the Abbé Nollet: the apparatus may be seen represented in Plate IV. fig. 10 and 11. It consists of a small bottle AB with a long neck, which is emptied of air and hermetically sealed: the neck, which in the figures is represented as almost naked, must be understood to be almost entirely inserted into a little tube of tin. As soon as this tube is joined to the Chain, copious rays are seen throwing themselves from the inside of the neck of the bottle thus coated, towards the bottom of it. If a hand E be applied to the outside, in B, the rays then fly towards it, and follow each other more vivid and continued than before. If the bottom be outwardly coated with a sheet of tin up to AD, where the neck begins (Fig. 11.) the rays of electric fire appear still more vivid in the naked part of the neck; and whether the naked bottle (Fig. 10.) be held in the hand, or the coated one (Fig. 11.), a spark flies out when the other hand touches the little tube FG; but the stroke given by such spark is more violent when drawn from the coated bottle; and the light with which the neck of the bottle is then replenished is also more vivid and copious.

265. I think that it will be sufficient to subjoin here the explanation of those *consecutive* charges and discharges, as I gave it in my letter to the said Abbé, in the parag. 486, 510, of my

my *Artificial Electricity*. I. The excessive fire of the Chain flows through the substance of the little tube, to the outside of the neck of the bottle which is within it. II. This excessive fire drives from thence an equal quantity of natural fire: the first and immediate charge is therefore produced. III. That natural fire, thus driven from the inward surface of the neck of the bottle, accumulates itself on the bottom, correspondently to the hand, or any other more ample body that touches it outwardly; and IV. drives into that body an equal quantity of fire from the bottom of the bottle: this is the second charge; I call it, *of consequence*. The explanation of the whole is, in short, that the neck, which is coated with the little tube, and the bottom which is surrounded by my hand, are like two different bottles, or plates of glass, mutually separated by the portion of the neck that is left uncoated; and they inwardly communicate together by the means of the vacuum. Let us now proceed to the discharge:—if, while the electricity is excited, the person who experiments, touches the coated bottom, or the same being uncoated, grasps with his hand the whole capacity of it, and then carries his other hand to the little tube, then the two discharges will be effected at once with a single stroke: that is to say, the excess accumulated on the outside of the neck of the bottle, flies through the body of the man who makes the experiment, and supplies the deficiency raised on the outward bottom of the same; and the excess accumulated on the inward bottom, runs, through the vacuum, into the inward surface of the neck; and it is that quantity of fire that makes, at that instant, the neck of the bottle appear as if full of light. With this experiment also the Abbé pretended to prove that the electric fire passes through the substance of the glass.

266. The same explanation may be given of the fine experiment commonly attributed to the celebrated Mr. Canton, which is made in the vacuum of a barometer. This is the manner in which I execute it. I fill with mercury the incurvated tube ABC (Pl. VI. fig. 2.) forty inches high, and I use all the necessary precautions to obtain an exact vacuum: the arm CB is short, so that, when I have erected again the tube (which I do very slowly, for fear of making the mercury oscillate), it remains full; and I apply to the opening of it C, a piece of melted sealing-wax.

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I then make the top of the barometer touch the Chain, while I grasp with both my hands the body of it, where it is full of mercury. The light that flows within the vacuum AB, affords a most agreeable spectacle: but when I have removed the barometer from the Chain, the interrupted flashes of light, which continue for several minutes to sparkle, and successively grow more languid and separated from each other, afford a still more admirable sight. Now every body sees that when the barometer communicates by its top with the Chain (I use to coat that top with tin, and hang it by a hook to the Chain) the excessive fire accumulates itself on the surface of it as far as it can spread itself, and drives an equal quantity from the inward surface; which fire goes first through the vacuum, and thence through the deferent mercury, to accumulate itself on that part of the inside of the tube which corresponds to my hands, where it can in its turn dislodge an equal quantity from the outward surface of the glass; and thus, are the two consecutive charges completed. When I afterwards remove the barometer thus doubly charged, the two charges tend then to destroy each other: the fire accumulated in the inside correspondently to my hands, strives to diffuse itself through the mercury and the vacuum, into the superior inward surface, from which it has been at first expelled; it, therefore, endeavours to expel the fire outwardly accumulated in that place; and, in the meanwhile, it draws the natural fire to that outward part of the tube by which I hold it. While such an equilibrium is gradually restoring, the transient fire lightens through the vacuum. In fact, it suffices that I touch with one hand the top of the barometer, and with the other, the sides of it, to make the fire after it has ceased to lighten, sparkle again.

267. The experiment which I described in the parag. 346, of my *Artificial Electricity*, is more complicated, and surely is not less agreeable, "A thin bottle of glass, I said, accurately emptied
" of air and hermetically sealed, as soon as it is immersed in the
" electric atmosphere of the Chain, appears full of electrical light
" not unlike those lights which, in a summer evening after a hot
" day, are seen about the horizon. Even such glass after it has been
" removed from the Chain, continues for some time to afford in-
" terrupted flashes." In that experiment, I only mentioned one
charge,

charge, whereas there are in reality three, viz. one of the air, and two of the glass, consequent to that of the air. Let the reader see the experiment represented in Pl. VI. fig. 4. The first charge is that of the portion of air which stands between the conductor Y, and the surface near to it of the glass BCD; the second is, that of this same part of the glass BCD, to which the immediate action of the electric atmosphere reaches; and the third is that of the glass in EFG, where it communicates either with me, or any other deferent body. I. The excessive fire of the conductor flows, in A, into the contiguous air and adheres to it, (I shall prove in the next chapter that air is capable of taking a charge in the very same manner as other *insulating* bodies) and drives an equal quantity of fire from the surface of the air which touches the glass in BCD. II. Therefore, this fire, from the surface of the air, accumulates itself on the contiguous outward surface of the glass BCD; and drives an equal quantity from the inward corresponding surface of the same: now, it is this fire, which, flowing through the vacuum, emits those transient flashes of light we have mentioned. III. The same fire is determined to run to the inward surface EFG, where it can expel an equal quantity from the outward surface, into the deferent body with which the latter communicates. When I afterwards remove the bottle from the Chain, the flashes that appear in it are exactly similar to those that take place in the barometer, and are the consequence of the motion of the fire, which recovers its equilibrium.

C H A P. V.

On the causes that lessen, or prevent, charges.

268. **I** Think I have treated pretty largely of the *communication* which must be between the two opposed surfaces of an insulating body, and two different systems (the one animated, the other indifferent; or both animated, but in a contrary manner *);

* The two systems may be both animated, but in that case, the charge will only be proportioned to the excess of the greatest homologous electricity above the other.

in order to produce a charge. I likewise think that I have sufficiently demonstrated that the thinness of the glass increases the intensity of the charge. I, for instance, never could succeed to introduce any perceivable charge into a Bologna flask; I mean one of those which are of a great thickness, especially at their bottom, and which break to shivers by a stroke from a little piece of a flint dropt into them.

269. But many things still remain to be observed with regard to the *separation* which must obtain between the two opposed surfaces of a plate of glass, in order to complete its charge. For as this charge cannot be effected but proportionally to the quantity of contrary electricities which accumulate themselves on the two surfaces of the plate, so this charge will continually lessen and vanish in proportion as there will be a communication between these contrary electricities; since, by their thus communicating together, they will mutually destroy each other, and consequently destroy the charge of which their *opposition* constitutes the essence.

270. Now, the two opposed surfaces of the insulating body can thus communicate with each other, either through the ambient medium, or through the uncoated edges of this same body, or even through the substance of that body itself. Of these three kinds of communication, as well as of such other circumstances as may obstruct a charge, I propose now to treat in order, and proceeding by experiments. I shall afterwards mention the precautions that are to be used in order to prevent these inconveniences.

271. And first, with regard to the air, or ambient medium, I rather choose to appear tedious than forget to mention, that, for want especially of a sufficient dryness in the same, there are but few days in the year in which thorough charges can be obtained: the sparks are often pretty vivid, and loud enough; but, for all this, the charges do not attain their utmost intensity. The experiments must be made in those few days of the year in which a strong wind prevails, and the sky is very clear (here, in Turin, such dry winds chiefly blow from the West, and rums adjacent to it): then the sparks of the discharges will produce effects greatly superior, either in melting, or vitrifying, or calcinating bodies, to those produced in ordinary weather. A single observation may confirm this: let
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the electroscope annexed to the Chain is seen immediately to attain its greatest divergency, as soon as the insulating body, which was charging, is removed; and the electroscope annexed to the Chain is seen immediately to diverge to the utmost: a most certain sign, this, that some moisture from the air adheres to the uncoated edges of the said body, and was, before the removal of it, as a kind of vehicle, by the means of which the excessive fire of the Chain flowed into the outward coating, and thence into the ground. Nor is such moisture deposited on the glass, only in the same quantity as it is spread in the atmosphere, but the electricity draws it, and disposes it in the properest manner for it to communicate itself through it, as we shall see when we come to speak of the electrical motions. I have said before how I partly obviate this inconvenience, by placing under the *Franklinian square* some hot ashes; but still I insist, that art never will supply, in this case, the advantages of a proper season.

272. But if moisture can thus effect a communication between the two coatings of a plate, how can it be therefore, that discharges of a pretty great intensity, are sometimes obtained in weather imperfectly dry? for it sometimes happens, that in charging a glass, or even an electric battery, that is an aggregation of several glasses, though in cloudy weather, the electroscope actually reaches its greatest divergency, and the charge happens to be really complete. I answer, that the vapours in the air may sometimes chance not to form on the margin of the glass such a continuous coating, or rather fur, as to enable any considerable portion of the electricity to dissipate itself through it; therefore, a strong charge may then be effected, that is, one which is proportioned to the excess that has really been accumulated and preserved. The considerable *sum* of the intervals, however small they may be, between the particles of moisture, produces in such case a sufficient *sum* of resistance to stop the greatest part of the electricity which is destined, by its accumulation, to form the charge. Thus, I charge a bottle, from the outward coating of which hangs a small chain of rusty iron; and the discharge as well as stroke are but weak: the links of the chain, we must here observe, only touch each other in points, and even in these, the contact is rendered imperfect by

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an interposition of the rusty, and consequently *incompletely deferent*, surface: now the consequence of these many little resistances is, that only a small quantity of fire is transmitted, and the bottle consequently is in the issue but weakly charged.

273. With regard, besides, to such complete charges as are sometimes effected, though the sky may be darkened with clouds, I have observed that they generally take place during two particular kinds of cloudy weather, viz. in times of temporary transient clouds, and still more frequently in times of snowy clouds. When a temporary transient cloud passes and lightens above our heads producing an almost incessant noise, and keeping at a little distance from the ground, in such case, and before it rains, the electric experiments succeed pretty well, because these clouds being strongly electrified, draw to themselves the other vapours spread in the air (even in rooms, the air is pretty soon brought to the same state in that respect, as the outward air) which they incorporate with themselves, and leave the air that lies near the ground, free from any deferent dampness. Also, when copious and dry snow falls, the electricity is extremely vivid. I explained the increase of the particles, or grains of snow, by the attraction that takes place between them, when they pass through clouds unequally electrified. The fine experiment with which Mr. Heberden proves, in the LIXth Vol. of the Philosophical Transactions, that of two vessels placed in the same vertical line, that which is lowest receives a greater quantity of rain than the other, renders it manifest that rain itself increases in its falling; I shall introduce in another place some observations concerning rare, and, at the same time, large drops of rain, which I found to have electrified an umbrella of tin, insulated. Now, since we know that kind of force which produces the mutual attraction and agglomeration both of the particles of hail and rain, why should not snow likewise draw the vapours diffused in the lower part of the atmosphere? a dryness even greater than in the former case, must in this case obtain, since snow can reproduce no new moisture in the air.

274. I have hitherto mentioned only that moisture in the air, which spreads itself on the uncoated margin of the insulated body, and produces the abovementioned decrease of the electricity in the
Chain

Chain at that instant, when the insulating body, destined to be charged, is applied to it; but the electricity in the Chain can besides receive a diminution from any dampness that may take place in the bodies that are made use of to insulate it, such as silk strings, pieces of glass, or oiled wood, and especially in the ambient air: so that the electroscope may then, even though the insulating body be not annexed to it, rise but little, and the intensity of the other signs be proportionally diminished.

275. If the dampness were copiously diffused in the air, the signs might even fail intirely. Attempting once to make experiments in a place in the country which was surrounded by growing plants, I found I could not succeed to excite any strong electricity; which I attributed to the copious transpiration of vegetables. The same will happen in all small confined rooms, where many persons are present. If fire be lighted, it will drive out the damp air, and draw in new and dry air, by which the electricity may be kept up with some degree of vigour. It is only to the copious dampness he must have produced, that I impute that Dr. Priestley, after filling a glass bell with air that had passed through his lungs, and immersing in it a charged bottle, saw the same discharged in a less interval than two seconds (*History of Electricity*, p. 599.) The moist vapour, of which the air in the glass bell was replete, became a conductor continued enough to enable the charge to pass from the hook, to the coating of the bottle; though, at the same time, the intervals, however small, between the particles of the deferent vapour rendered the passage of the fire slow enough to prevent a spark. When I want to suppress a charge for which I have no farther occasion, I lay the bottle on an old table of walnut-wood, so that the hook touches the table, and it discharges itself there silently, though speedily, and without giving me any farther trouble; the remnant of deferent particles in this table is sufficient to dissipate the charge, but their separation, as well as nature, occasions such discharge to be only silently effected.

276. Dr. Priestly has filled the same glass bell with air proceeding from a fire of wood; and a bottle immersed in it has been in a like manner discharged: nor has the same gentleman ever been able to charge the said bottle when keeping it immersed in such

air : but the air being changed, the bottle kept its charge if already charged, or received one, if it had none. (Ibid. p. 600.) Dr. Priestley has found afterwards that coals of wood, and pit-coal, were conductors of electricity. He calls them, excellent ones; to me they have appeared inferior to metals, because though they may conduct the spark to the same distance as they, and leave no greater remnant of charge; yet, they do not conduct the spark so united and compact; the lightning of it is not so vivid, nor the crack so loud, as when it is conducted by metal.

277. Whatever may be the real consequences to be drawn from Dr. Priestley's experiments, I think, I perceive from them, a new kind of analogy between the common and the electric fire, as well as discover a probable explanation of a very much known phenomenon, concerning the former; which is the gradual extinction of a flame in air impregnated with vapour from the lungs, or from burning coals, or in any other mephitic air. Since my object in this book is to promote the knowledge of Nature, it will not be amiss, I think, to expose here how I explain the gradual extinction of a flame in air that grows continually more and more dilated. I consider such an extinction of the flame as a real dissipation of it:—in order to enable the flame to live, it is necessary that a certain portion of it be continually reperfused, and kept close to its *pabulum*, in order that it may loosen, and continually put in motion, new particles of it, and disclose the fire imprisoned in them, in order to supply that which continually dissipates. Now, air is the medium that reperfuses and confines such portion of fire: in proportion as air grows dilated, the fire is the more copiously dissipated; only a less portion of new one is then disclosed; the flame thus gradually lessens, and at last fails, after draining the cavity of the candle around the wick. Now since air, impregnated with vapour, either from the lungs, or from burning coals, is able to conduct the electric fire, why should it not be able likewise to conduct common fire? this circumstance, if it were well ascertained, would give a new degree of extent to the already known analogy between those bodies which are *deferent* of both the said elements; though we must observe, they perhaps are but one and the same element, which is only differently combined.

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Farther, since a flame dies away in rarefied air, in consequence of its dissipating too much in it, why should it not likewise die in air filled with vapour from the lungs, since through them it can as well dissipate, as through a vacuum? But of this we shall say more hereafter.

278. Now it is necessary that I should proceed to consider the different kinds of communication which (besides that from moist air, or any deferent medium) may be afforded by the surface of the insulating bodies on their naked margins: I think that they may be reduced to four principal ones, viz. to a too great narrowness of these margins; to a want of cleanness; to a defect in the shape and clipping of the coatings; and to the bad condition of the deferent body, which may prevent it from insulating exactly.

279. With regard to the three first cases, we shall say little. 1. For the charging of the largest batteries, a margin of three inches on each side of the insulating body is sufficient; this being a distance which no charge, ever so strong, has yet being known to be able to go through. 2. But such margins must moreover be exactly insulating; that is to say, clean. Whatever particles the electric fire may meet with, which it can drive into its own way, it will actually drive, and will make them, as it were, so many points of support, by the means of which it will leap, and successively reach, to a great distance: this I have shewn before in my Book of *Terres Atmos. Elec.* and will more amply shew hereafter, in the chapter on the Electric Sparks: I therefore again insist, that the naked margins must be kept exactly clean. 3. In order that the aforesaid distance of three inches may suffice, care must be had that the angles of the coatings be not sharp, else they will throw out particles of fire. See, for instance, what takes place in the coating of one of those plates, *f, g, h, i*, which I call *Franklinian squares*, when charged in the dark: in proportion as the charge advances, brushes of light spring to a great distance from each of the angles *f, g, h, i*, and fringes of light line the sides of the said coatings: now, all this quantity of fire is so much that is taken from the charge. This inconveniency we may indeed remedy by rounding the angles of the coatings; but the best remedy of all is to apply

apply around them a coat of sealing wax; thus the dissipation of the electric fire through the sharp sides, and much more through the angles, of the coatings, will be in a great measure prevented, as we shall see hereafter, when we shall treat of the electric *brush* and *star*: whatever plate I have fitted thus, has been able to receive a stronger charge than others of a same size. The above-mentioned brushes and springs are by no means a portion of fire useless to the charge: in fact, long after they begin to appear, the charge still continues to increase, and no doubt but it would increase still farther, and at least be formed much more speedily, if such dissipation of the fire were prevented beforehand. The care of smothering, or suppressing, such brushes of light, must also be extended to the whole body of the Chain, of the rod, and any other body, by cutting or filing off all prominent parts on them. Any moveable and deferent particle on either of the coatings, would also greatly lessen the charge: an old experiment of mine may here serve as an example: I had placed a stripe of tin-foil close to the coating of a plate; and when the spark rushed to the *conducting* bow, it carried the stripe along with it, and dissipating it in smoke, imprinted some of its particles on the bow.

280. But the principal cause of the dissipation of the electric fire, is the fourth we have mentioned; viz. the bad condition of the insulating bodies: which, in some, is perpetual; in others, transitory. It is perpetual in some glasses in consequence of the particular stuff of which they are made; and transitory in other glasses, which, though their substances may be excellent, yet do not insulate, either because they are new, or, though they may be old, have been lately heated somewhat intensely.

281. Dr. Priestley (p. 616) coated a bottle of blue glass, near its bottom, to the height of an inch and a quarter; the bottle was six inches and an half high, and two inches and an half wide; and, as the charge was advancing, the sparks began to fly from such naked places of the glass as were near the coating: then they successively flew from higher points under the shape of brushes that became gradually larger; and lastly, they ran from the inside of the bottle along the outside of it; and thus the discharge was ef-

effected. The bottle was afterwards also discharged, though the coating was reduced to half an inch: and even another similar bottle filled with filings of iron, was discharged without any other coating than a little iron chain which fastened it to the table, near its bottom.

282. Dr. Priestly observes, that the first of these bottles which was broke only eight months afterwards, retained still the same property: I shall observe here, that the progressive motion of the sparks on the uncoated surface of bottles like those mentioned here, till a discharge is effected, only differs in point of quantity and swiftness, from the ordinary phenomenon observed in the charging of bottles, the surface of which are not, in all their parts, equally deferent. When, for instance, the bottle A B is charging at the Chain, (Pl. II. fig. 5) little sparks soon begin to issue from such parts of the glass as stand near the coating; which sparks successively grow larger, and sparkle from the distance of half an inch, an inch, an inch and an half, or two inches, and even sometimes more; but for all that, a discharge does not follow, because the glass, if it be good, well polished, and dry, is not able to transmit the spark to any greater distance. To render the experiment still more conspicuous, I use bottles with a long neck, and I coat them with a stratum of cement, which is a substance still less deferent than glass; to which add, that such an addition to the former thickness of the bottle, will lessen much the inward force which drives the sparks from the outside of it: now, it is this very force which is the cause of that phenomenon which Dr. Priestley calls a very beautiful one. The excessive fire, in proportion as it accumulates itself on the inward surface of the bottle, correspondently to the outward coating of it, successively exuberates, both in the filings, and in the iron wire used by Dr. Priestley, where they respectively correspond to the outward uncoated surface: consequently, such fire in proportion as its density increases, drives portions of fire from successively remoter places of the uncoated outward surface, into the coating of the bottom of the bottle. And as to the bottles used by Dr. Priestley, made of a glass the deferency of which is of a particular nature, the same excessive fire, when it has attained its greatest density, becomes able to rush, one part of it, into the
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the outward coated surface, which is deprived of the greatest part of its fire; and, the other part, into the uncoated surface, into which a deficiency has been raised, proportioned to the sparks that have also been at times driven from it.

283. The same explanation may serve likewise to illustrate, if not the cause, at least the manner after which those phenomena are effected, that are observed to take place in those glasses, which, on account of their being new, cannot, for a while, insulate but in a very inconsiderable degree. When I examined at first the experiment of the abbé Nollet, described in fig. 9. pl. III. I inserted an old bottle B, in the neck S of an old glass bell C E D, and I remained not a little surprised that I could not perceive within this bell (while indeed all the rest of the experiment succeeded extremely well) that great quantity of fire which the abbé said filled the recipient, during the time that the bottle was electrified, and moved in it with a stupendous rapidity: in a word, I was unable to discover any thing like the beautiful, and, indeed, true delineation that he gives of such phenomenon, in the first plate of the first volume of his Letters. I procured afterwards another glass bell from the furnace; the height of it was a full foot, and it was consequently more nearly equal to that used by the abbé; and when I repeated the experiment, I saw flashes of light like those observed before by him, winding about in a most extraordinary manner: but the very same evening, having applied my hand to the glass bell, while with the other I tried to effect a discharge, I was vehemently struck, and the bell broke, it being made of a thin glass. I procured another of the same size, which exhibited to me the same spectacle; and I laid it in a place where, upon occasion, I could find it again.

284. But I must confess I was amazed, when about a year after, intending to repeat the same experiment, I found myself unable to raise those strong and winding flashes that before filled the glass bell; and I could not see any thing more than an almost single ray, which, from the outward bottom of the bottle, flowed to the knob on the plate of metal on which the glass bell lay. Then, for the first time, the thought occurred to me that white glasses, when recent, are in some degree conductors of the electric fire;

fire; the reason I had for thinking so, was, that the flashes, which moved in a vermicular line, did not possibly flow through a vacuum, since, as I had before observed in numberless experiments, they always proceed in it, in a right line, or evenly incurvated line: the vermicular motion of the flashes, just mentioned, must therefore proceed from their running in a contact with the interior surface of the glass, where they follow such crooked path as is afforded them, by such parts of the glass as actually are, or successively become, *deferent*, in consequence of the electric fire affixed to the parts next to them. I imagined besides, that this defect of new glasses becomes, in a course of time, gradually less; and that this was the reason why the flashes failed within an old glass bell, and afterwards in the new one, when I had left it grow old. I thought that this defect was confined to white glasses; because cylinders, or globes, of greenish glass, the same as that of which bottles are made, though they were newly made, had served me very well at all times to excite electricity. With respect to this my opinion, I think, I pretty well agree with Dr. Priestley, who has afterwards made many experiments relative to the *deferent* quality of recent glasses. He has especially made his experiments with tubes of flint-glass, and has found them, in some degree, *deferent*, when they were new. I. Whether they were thin, or thick: II. whether they were polished, or rough: III. whether they were hollow, or solid. IV. The same gentleman has found, moreover, that glass, however recent, becomes continually more and more *deferent*, by being several times charged and discharged; but that, after a few hours time, it returns to its former state: V. that a space of a few months, after the glass has been taken from the furnace, and using it, are necessary to render it insulating: VI. that, when they are once become insulating, they may again be rendered *deferent*, by being exposed to the action of a fire sufficient to render them soft and pliable. (P. 621.)

285. I generally used to bespeak tubes of glass, two or three yards long; I took them hot from the furnace, insulated them, and found that at the same time that I applied a charged bottle to the end of one of them, the electroscope, annexed to the other end, diverged, and a pretty strong charge was communicated to another bottle, annexed to the same end: but, a few months after, I found

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that the fire did not diffuse itself through the same tube, to a greater distance than half a yard.

286. I applied a coating, three inches broad, to the middle of a tube, open at both its ends, and a yard long; I charged the coated part of it by the means of an iron wire, when it seemed as if a flame sprung out from both sides of the tube; and sparks ran to my fingers, as I moved them along it, from all the successive parts of its surface.

287. I took another tube, an inch in diameter, three feet and a half long, and extremely thin, and applied to it a coating three inches broad, at the distance of nine inches from the one end. When the coated part was charged, the whole surface of the glass threw sparks to the distance of an inch. In moving my hand along its outward surface, I destroyed its visible electricity. By presenting my finger to one of the two openings, I diminished the inward electricity, and again excited the outward one; and, by proceeding thus at several times, I effected a discharge by *alternation*.

288. The finest experiment of Dr. Priestley has been that, in which he applied a coating, three inches broad, to the tube A B, in the same manner as is expressed in the Pl. VI. fig. 3; then inserted an iron wire E, in contact with the interior coating, and charged it by joining the wire to the Chain. This done, he took the coating C D, and moved his other hand along the tube from B to D, as if he had intended to rub it. When his hand came to be distant from the coating only two inches and an half, a spark flew from the latter into his hand, which produced a shock: sometimes Dr. Priestley was obliged to move his hand, even three times, from B towards D, before he could obtain a stroke; at other times he was struck at the first motion; and sometimes he even received a farther stroke at his third motion.

289. In order to explain this kind of strokes, Dr. Priestley made use of a smaller tube, within which he had made a perfect vacuum (this experiment is the same as that which I have explained in No. 486, 518, of *Artificial Electricity*), and pretended that the charge and discharge of that tube being explained, would throw a light on the new method of obtaining a shock from tubes in some degree deferent, within which no vacuum had been effected. The

truth is, that such a discharge is only a discharge by *distribution*. I take a tube A B, which is exactly insulating (Pl. VI. fig. 3.); I charge it in C D, after having coated it in the same manner as Dr. Priestley did his own: then I apply my hand in F, and another person puts into the opening B of the tube, a rolled piece of tin-foil, like that used for looking-glasses; this piece of tin-foil is made to move forward and backward, within the capacity of the tube, by the means of a stick of glass to which it is fastened. This done, two sparks are thrown, the one inwardly, from the inward coating C D, toward the piece of tin-foil (which, as I suppose, is made to accompany the motion of the hand on the outside); the other, from the hand to the outward coating. By the former spark, the excess becomes divided, that had been at first introduced into the inward coated part; and by the latter the deficiency of the charge raised on the outside of the same coated part, is lessened. In fact, if I take my hand from F, I find the tube (the excess introduced into its inside being a little diminished) electric in that place by deficiency.

290. I think, the stroke produced by Dr. Priestley was of the same kind. I. He introduces, into a recent tube in C D, outwardly an *excess*, inwardly a *deficiency*. The hand applied to F, enables a portion of natural fire to run to D C, and thus to lessen the deficiency in it; because the excess, which is raised on the inward surface in D C, then becomes able to diffuse itself into that part of the same inward surface which lies next to it and corresponds to the hand outwardly applied in F. But we must observe, that such distribution of the inward excess is facilitated both by the nature of the tube, which is in some degree deferent, and by the motion of the hand along its outside, from B to F; the effect of which motion is, that the excess in B can run to the hand, while the latter stands near; and the excess that remains in those parts which are nearest to F, can inwardly flow towards B, as the hand moves towards F.

291. We may, however, conclude this from the above experiments, viz. that recent glasses are able to retain a certain degree of charge, and that they consequently do oppose some resistance to the fire that endeavours to flow along their surface.

That such resistance becomes successively greater, in proportion as those glasses grow older, and are more used; since a greater force and density of the electric fire is then required to surmount it. The consequence to be drawn from all this is, therefore, that glasses then lose less of their fire; lose it only at a smaller distance, and can retain stronger charges.

292. Dr. Priestley concludes his ingenious section with the following experiment. He put a slip of gold or brass leaf between two tubes, the one old, the other new, and strongly fastened to each other; and observed that, when he had happened to draw very strong sparks, the metallic particles were more indelibly impressed on the new tube than on the old one, and that such impression was manifested by a more beautiful colour: which difference, he conjectured, was owing to a difference in the pores, which were more open in the new glass; and as the same grows old, become successively contracted.

293. But to speak of what I have myself observed, I ingenuously confess, that the finest experiment I have been able to see; with regard to the deference of recent glasses, is that which I have described in numb. 285, that of the vermicular flashes which filled the bell made of recent glass, which was extremely brittle, even beyond what might be expected from its thinness. In other white, or greenish glasses, I could not perceive such a great deference as Dr. Priestley has observed in his crystals.

294. I bespoke several tubes of white glass, and some of them were scarcely made when I exposed them, still hot, to the open air, which was cold to the degree of congelation; and a few others of the same size I got to be heated again in the furnace. I tried them all the same day, and found that they diffused away, in some degree, the electricity of the Chain; but this they did not, either with so long flashes, or so plentifully as the tubes of Dr. Priestley did: though I observed that such tubes, as had been heated twice, lost their electricity in a less degree than those which had not been. I have repeated the same experiment with tubes of greenish glass, the one heated a second time, the other not, and the effect has constantly appeared to me to be the same.

295. Dr. Priestley says, at p. 622, that some tubes, that had been made the same day could not be electrified, though rubbed ever so much.

much with the amalgamated cloth. With me, quite recent tubes have become a little electrified, when rubbed; and if a little heated, have manifested a pretty considerable degree of electricity; though they only retained it for a very short time.

296. I say, a little heated, because a great degree of heat is itself the last of those transitory causes mentioned above, that render glasses *deferent* on their surface; and is almost the only one that makes them pervious to the electric fire, in their inward substance. Before I examine the effect produced by this last cause, viz. heat, I will explain why I say, it is *almost* the only one that renders the substance of glass pervious to the electric fire. Ever since the experiment of Leyden has been known, attempts to charge thin Florence flasks, or large matraffes, have always proved unsuccessful: I attributed it at first to the number of bubbles remarkable in that kind of glass, which, by dividing the thin substance of it, left some continuous openings in its inside, that gave an access to the fire. But the opinion of Mr. John Canton, on that subject, is no less probable: he thinks that those little spots on the glass are formed by metallic particles, which, not being sufficiently melted, afford a passage to the subtle elements. Indeed, it does not appear that thinness may of itself be the cause of the penetrability of glass: I have blown with a lamp small bottles of glass, and made them as thin as they possibly could be, and still they were able to receive most intense charges.

297. Let us now examine the effects produced by an actual degree of heat in the glass: in the Letter I wrote to Dr. Franklin, the 20th of February, 1767, I expressed myself in the following terms, in numb. 33. *When I try to charge a glass very intensely heated, sparks are continually thrown from all parts of it: we must therefore conclude, that the fire either circulates around such glass, or passes through it: I have an experiment ready to ascertain this.* Here is then the experiment, as I made it. I procured a bottle B C D E (Pl. VI. fig. 5.), the neck of which was crooked, and eight feet long; I put water into it, so as to fill only a sixth part of its bottom, and made an iron wire dip into it. I insulated the same by two strings of silk, fastened to it in C and D; I shook the bottle, that the water in it might wet all the inside of it, and made:

made the hook communicate with the Chain. When I grasped the bottom of the bottle, the electroscope rose to a great divergence; and when I afterwards touched the Chain with my other hand, I received a strong stroke. I then put a lamp under the bottom of the bottle, which soon made the water in it boil, and, in such state of things, the electroscope diverged no more. I insulated the lamp F, when the electroscope immediately diverged; and at the same time, I could continually draw sparks from the metallic prop of the lamp; but when I intirely touched it, the electroscope immediately lost its divergence. This is to be concluded therefore, viz. that the electric fire passed through the bottom of the bottle, when heated to the degree of boiling water. It was only after making this experiment, that the similar one, of which Mr. Kinnerfley wrote a description to Dr. Franklin from Philadelphia, came to my knowledge. "I put," says he, "boiling water into the coated Florence flask, and found that the heat so enlarged the pores of the glass, that it could not be charged. The electricity passed through as readily, to all appearance, as through metal (a): the charge of a three pint bottle went freely through, without injuring the glass in the least. Would not this experiment convince the abbé Nollet of his egregious mistake? For when the electricity went fairly through the glass, as he contends it always does, the glass could not be charged at all."

298. Even before I had received the complete edition of the Works of Doctor Franklin, I had heard of the experiment made by lord Cavendish, related in page 403 of them. I do not think, I have perfectly well understood the particulars of that experiment: however, I shall relate in what manner I have tried to imitate the essential part of it; and, if I do not mistake, carried its consequences even farther. I. I took a small tube of glass CD (Pl. VI. fig. 6.) of white glass, three lines in diameter, the fifth part of a line thick, and three feet long, through which was passed an iron wire GH. I then made the tube go through the two pieces of paste-board that served to close on each side the small vessel of brass AB,

(a) We shall see, however, that though the degree of heat be higher than that of boiling water, glass never is so deferent as metal is.

which

which was two inches broad on every side. II. That is to say, I placed the two pieces of paste-board (they are not expressed in the figure, to prevent a confusion) in the vase A B, in such a manner, that the tube passed through them, and also through the two openings, made, facing each other, in both sides of the vessel; and then I moved the two square pieces of paste-board from each other, so as to make them cover pretty exactly the two openings. III. This done, I introduced into the vessel the ball of a thermometer emptied of air, which could rise to number 255 above 0 (a); and, keeping the same suspended, so as to have its ball on a level with the lowest side of the tube, I poured into the vessel as much iron filings as were necessary to cover the said ball. IV. I made the iron wire G H, that served as an interior coating to the tube, communicate with the Chain, and annexed an electroscope to it in H, which I call the electroscope of the interior coating. Lastly, I fastened to one side of the vessel the iron rod I K L, suspended by a silk thread, to which I annexed another electroscope, which I call the electroscope of the exterior coating; because the rod I K L, the vessel A B with the iron filings, and the table on which the whole lies, which is insulated, all belong to that coating which covers the outside of the tube.

299. Things being thus disposed, I. I excited the electricity of the Chain, and in the meanwhile I now and then touched the vessel, but after four or five seconds, could no longer draw sparks from it; and the electroscope L, of the outward coating, which at first was made to diverge by the natural fire that was driven from the outward surface of the tube, absolutely ceased to do so, though I continued to touch the vase; that is to say, in four or five seconds the charge was completed.

300. II. I then put under the vase a little lamp with spirit of wine, when I was not a little surprised to see that, after a few seconds, when the thermometer from 8 degrees (Fahrenheit's grad. 25 deg.), where it stood at first (the experiment was made the 17th of November, though a most excellent weather), had scarcely

(a) The author speaks here of a thermometer graduated after the method of Mons. de Reaumur; the number 255 of which amounts to number of Fahrenheit's scale.

got to nine, that the electroscope L began again to diverge a little, while the electroscope K of the inward coating manifested a tendency to fall. I several times tried to ascertain this circumstance, by repeating the experiment; and at every time I found that the addition of a single degree of heat is sufficient to make a charge already completed, rise somewhat higher: which may be owing to two different causes; either such a small increase of heat enables new and remoter parts of the surface of the tube to give some of their electric fire; or, this same small degree of heat enables that very part of the glass, which was at first as much deprived of fire as it then could be, to yield some additional one.

301. That the charge should increase after the manner first mentioned, is intirely conformable to experience: yet it appears to me no less probable that it increases also after the second manner, to wit, that successive additional degrees of heat enable continual new electric fire to leap from other more and more internal pores of the same part of the glass. Such hypothesis seems to be conformable to the universal law of *similar actions*, which never proceed but gradually and evenly through the substance of bodies; and it moreover particularly agrees with the nature of glass, which opposes the diffusion of the common fire in the same manner as we see it does that of the electric fire, and is not penetrated by it, but gradually and slowly: so that the dilatation of those strata which the fire has already penetrated, causes those which are still unpenetrated and contracted, to break. Such hypothesis would also satisfactorily explain the effect of the method used by some persons who, in order to increase the charge of certain bottles, fill them with water a little warm: it might in such a case be said, that such small degree of heat causes the electric fire to be more copiously and deeply condensed than it was before, within the pores of the one surface, and also to be driven more abundantly, and from deeper parts, from the other surface; without, for all this, passing through the substance of the glass, or at least without passing in such quantity as to compensate on the one surface, that greater quantity which is accumulated on the other. But that which particularly confirmed me in this hypothesis, was the farther progress of the experiment; because, III. it always happened

pened that, as the heat increased, the divergence of the electroscope L of the exterior coating increased, whenever it remained untouched; and whenever the same was touched, the divergence of the electroscope H diminished.

302. IV. When the mercury was not above the 20th degree, even then, if the vase was frequently touched, very sensible sparks were thrown out, and in those instants the electroscope H was falling down. To explain those circumstances shall we say, that they were not owing to the charge which did only reach successively deeper, but to the fire that fairly passed through the substance of the glass itself? Yet, charges often succeed, and are exactly completed (that is, sparks from the outward coating are often seen intirely to cease) in summer days, though the thermometer rises to 22 degrees, or more; that is, to a degree of heat superior to that which obtained in the above experiment.

303. V. From this experiment I could conclude no sure method of determining nearly enough, at what degree of heat the electric fire really traversed the substance of the glass. At 25 degrees, when I constantly touched the outward coating, the electroscope H of the interior coating was already considerably lowered, and only rose again to its greatest divergence, when I left touching; and then, in four or five seconds, the electroscope L acquired a divergency equal to the greatest of the electroscope H.

304. VI. This at least I have concluded, viz. that a degree of heat, much beyond that of boiling water, is requisite to render a glass as pervious to electric fire as metal is. When the action of the flame had lasted twelve minutes, the mercury rose to 140 degrees; yet, when I then touched the exterior armature, two seconds were necessary for the electroscope H to lose its divergence; nor did it even completely lose it, though I constantly touched the said coating, but always was somewhat attracted when I presented my finger to it: whence we must conclude, that even at such degree of heat the electric fire did not freely go through the glass.

305. Every part of the apparatus being grown cool, I substituted another tube made of the same kind of glass, but of a thickness double to that of the former; and I then found, 1. That the charge, when every thing was in its natural state of coldness, was soon

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completed; that is to say, in two or three seconds, conformably to what has been observed before, that thick glasses can only receive a less charge. II. The sparks from the exterior coating have taken more time to appear with a certain degree of vividity. III. And lastly, it only was after twenty-four minutes of a continual heating, when the mercury had risen to 190 degrees, that it appeared to me that the fire passed freely enough through the substance of the glass: since that time, whenever I continued to touch the outward coating, the electroscope H, of the inward one, lost nearly its whole divergence.

306. Lastly, I tried the same day a third tube, a twelfth part of an inch thick, and of a greenish kind of glass; and I became still more confirmed in the opinion, that glasses of different degrees of thickness never are traversed by an equal quantity of electric fire, but when those, which are thickest, have contracted a greater degree of heat than the others: in fact, the mercury had already risen to 200 degrees, when, though I constantly touched the electroscope L, the electroscope H still retained a pretty considerable divergence.

307. When I attempted at first this kind of experiment, I used short tubes, and a rod K L, that likewise was only some few inches long: I likewise made use of a lamp with oil of olives, and made the experiment in a close room: each of which circumstances obstructed the success of the experiment; for, even though I continued touching the outward coating, the electroscope L, annexed to it, continued to diverge. I observed that, when I began to touch the coating, the electroscope first fell down, and then began to diverge again, though I still continued to touch; whence I perceived that it did not diverge in consequence of its own electricity, but because it was immersed in vaporous electrified air. I concluded, therefore, that a flame from spirit of wine, by yielding vapours less electrifiable, would be fitter for my purpose: that, by using longer tubes, and a longer rod I K L, the electroscope L would be more distant from those bodies that electrified the vapours in the room; and lastly, that, by keeping my instruments in the open air, all such vapours and electricity would be prevented.

ed. And it has only been, after all these precautions, that I could bring the experiment to its necessary simplicity.

308. But let us now draw some useful rules from all these observations and experiments. Certainly, if to the above mentioned effects of heat on glass, we join what has been explained and discussed in this chapter and elsewhere, I am greatly mistaken, if it does not give much assistance for the solution of the important problem of producing the greatest possible charges. I. Let an apparatus be procured of a Chain, Machine, and glass, that be most active, that is, that can procure the greatest possible quantity of fire in a given time. To that end, an apparatus of glass plates will be very useful, if it can be procured so as that each plate performs its own function with a sufficient degree of exactness, according to what has been said in number 52. II. Let glasses, or crystals, be chosen that be old enough, in which there be the greatest possible capacity and thinness, taken together; and in which a sufficient amplitude be combined with the least *perimeter* possible of uncoated margins: the reason of the latter caution is, because glasses, like air, seldom insulate thoroughly; therefore, the less the uncoated margins are, the less the number will be of points through which the two contrary electricities may communicate, and thus destroy each other. This is one of the reasons why in electric batteries the strength of the charges does not increase proportionally to the number of the glasses that are used, and why a vessel with a long narrow neck receives more vehement charges than a plate, even of a greater capacity: to this reason I have imputed those most vehement sparks, which I could draw from the stratum of cement with which I had coated the globe of brass (Pl. IV. fig. 5.). III. These margins must, besides, be kept extremely well polished; and when either they, or the other parts of the glass are to be warmed, care must be taken not to render them sensibly pervious to the electrical fire. IV. Armatures or coatings must be used, that are in the highest degree deferent, consequently made of metal; they must besides, (in those places particularly whence the spark is to be drawn), be of a sufficient solidity, so that the heat of the metal, which would otherwise be melted, do not injure the glass: the angles

must, moreover, be clipt, or, which is better, the perimeter must be lined with an edge of sealing wax, where it lies exposed to the open air (we shall see, in its proper place, what is to be understood by the words *open air*); and care must be had, that the systems, with which the coatings communicate, and which are therefore as it were a prolongation of the latter, be freed from any sharpness or asperity on their surfaces. V. The two opposed coatings must communicate with the two contrary systems, the Machine, and the Chain, that the two contrary electricities may concur in increasing each other.

309. It is true, Dr. Priestley, in page 584 of his History, indicates a particular method of increasing the charges. "I charged (says he) a tube about three quarters of an inch in diameter, and coated about eight inches, in the glass vessel, containing about two atmospheres,; and it received a much greater charge than it could be made to take in the open air, and as near as could be judged, by the report and flash, twice as great. At last the tube burst by a spontaneous discharge, after being charged and discharged three or four times, in the condensed air. It is not at all probable, that it could have been broke by any charge it could have held in the open air." But with regard to me, whatever diligence I have used, I could not succeed in the experiment above. Let the apparatus be the same as is expressed in the Pl. VI. fig. 7: it consists of an ample cylinder of glass, open at both its ends; it is placed standing, and a bottom of brass is fastened to it, with a solid ring of metal, and some very tenacious cement: to the superior opening let another very massive ring of brass be likewise fastened with cement; this ring must be so made, as to be opened and contracted at will, with a screw, and it will serve to fasten on the same opening a piece of leather, with a lid of brass: from the middle of this lid rises a strong tube of brass, supplied with a little key, to which is adapted the *compressing syringe*. Before I place the said lid, I put a jar of crystal, coated on its outside and inside, within the cylinder, as may be seen in the figure: this jar is placed on a sheet of lead, which makes it communicate with the bottom of the tube, and, at the same time, prevents its receiving any injury from a shock, or
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other accident: the jar is besides secured, laterally, by shavings of the same metal. When the lid has been placed on the cylinder, I open the top of the tube of brass which rises from it, and let down through it a thin iron wire, which is of a sufficient length to touch, with one of its ends, the bottom of the jar, and, with the other, lean against the inside of the opening of the tube. I have a number of times charged and discharged this vessel, thus inclosed, sometimes leaving the air around it in the natural state, sometimes doubling, or even increasing still more its density; and in all these cases the charges and discharges have appeared to me equally strong, and the light from them to exhibit exactly the same appearances, either with regard to its sparkling, or other motions. I have charged, at the same time, both the jar inclosed as above, and another glass of the same size and form, placed in the open air; and I have found that their two charges exactly balanced each other, which must necessarily be the case: whence we must conclude, that the electric fire cannot, though inclosed in condensed air, acquire a density superior to that of the fire, in a vessel which stands in the open air. It is very probable, that Dr. Priestley has made his experiment in extremely favourable weather, and the intensity of the charges which he obtained, have induced him to think that the charge of that glass, which stood in the condensed air, was doubled; and his having omitted to make the comparison, like that just mentioned, between the two charged jars, gives a new degree of probability to this conjecture.

C H A P. VI.

On the preservation of charges, and increasing of discharges.

310. **W**HEN, in the year 1753, I wrote to the abbé Nollet, concerning the imperviousness of glass to the electric fire, I ventured to tell Count Carburi, that I hoped I should once be able to send to Paris a bottle, hermetically sealed, and charged: but I happened, at that time, to operate in a very hot season, nor did I take sufficient care of the bottles; hence, out
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of many bottles that I sealed after charging them, one only could afford some remains of electricity, and those very small, two days after. This very same fact, though it rather confirmed me in my opinion, (I intirely imputed my want of success, to my want of skill in making the experiments) made me fear that the imperfection of the experiment would rather make the case worse with cavillers, than throw a light on the subject: but Mr. Canton, as if he had intended to relieve me from my fears, wrote to Dr. Franklin the 31st of October, 1760, in the following terms. "Having procured some thin glass balls, about an inch and an half in diameter, with stems or tubes of eight or nine inches in length, I electrified them, some positively on the inside, and others negatively, after the manner of charging the Leyden bottles, and sealed them hermetically. Soon after I applied the naked balls to my electrometer, and could not discover in them the least sign of their being electrical: but holding them before the fire, at the distance of six or eight inches, they became strongly electric in a very short time, and more so when they were cooling. These balls will, every time they are heated, give the electric fire to, or take it from, other bodies, according to the *plus* or *minus* state of it within them. Heating them frequently, I find, will sensibly diminish their power; keeping one of them under water a week, did not appear in the least to impair it. That which I kept under water, was charged on the 22d of September last, was heated several times before, was kept in water, and has been heated frequently since; yet it still retains its virtue in a considerable degree. The breaking two of my balls accidentally, gave me an opportunity of measuring their thickness; which I found to be between seven and eight parts in a thousand, of an inch. (The reader may see the complete edition of the works of Dr. Franklin, published in London in 1769, p. 401).

311. In consequence of this discovery of Mr. Canton, Dr. Priestley, the 30th of December, 1766, "examined a tube three feet in length, one half of which he had charged in the month of March preceding, and then sealed hermetically; but could not perceive that it was excited in the least degree, either by heating or cooling. The difference in the result of this experiment
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from several of Mr. Canton's, related p. 296, he attributed to the thickness of the glass tube. Mr. Canton charged small balls exceedingly thin. He also observed that there was no perceivable difference in the excitation of the charged or uncharged part of this tube, and that both parts acted exceedingly well. He afterwards opened this tube, and pouring a quantity of leaden shot into it, found it to contain a very good charge. It gave him one considerable shock, and several small ones; as he made no use of an outward coating, but only discharged it by grasping it in several places by his hand."

312. From these experiments we may conclude it as a certain truth; I. that glass, when it is not dilated by a certain degree of heat, does not conduct the electric fire. II. That glass receives a charge, or, in other words, contracts equal contrary electricities on its opposed surfaces, only so far as it is not traversed by the electric fire. III. Therefore, the action by which an electricity raised on one of the two surfaces, raises an electricity contrary to it, on the opposed surface, is not an *immediate* action, but is produced within the substance of the glass, by some vibration or pressure, or removal of some *support*, or in general, by some alteration or other, produced by the exciting electricity, either in the glass, or the fire inherent in it. IV. Lastly, the equality between two contrary electricities that constitute the charge, ceases to be permanent, whenever the glass begins to be penetrated by the electric fire. Hence, though when Mr. Canton heats his balls of glass, a deficiency at first takes place on the outward surface of them, equal to the excess raised in the inside, yet, this excessive fire will no sooner be able to pass from the inward to the outward surface of the ball, than it will remain without any effect; or at least it will only have that of filling up the outward deficiency, and no electricity will of course be manifested: therefore we may say, that in proportion as the heat begins to dilate the glass, the outward surface of it draws nearer to its natural state; also, that the inward excess passing through the glass, and at last accumulating itself on the outward surface, manifests itself there by the usual signs. In fact, that the glass once rendered penetrable, does not require any longer to possess the two contrary electricities, in
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order to its being able to manifest either of them, plainly results from what I said in my book of the *vindicating* electricity, p. 63. 64. I warm a plate so intensely, that I cannot hold it for a single moment, I present it to a rod that hangs from the chain, and it becomes electrified by excess in every one of its parts, just as if it was a body that were *deferent* by itself. V. It follows moreover, that if, in order to preserve the charges, Mr. Canton has been obliged to keep them even from the heat of an autumnal day, we may conclude, that the electric fire can be enabled to pass through glass, (though perhaps very scarcely and slowly) by a very inconsiderable degree of heat.

313. To proceed now to the object of this chapter, I observe that the glass, when thoroughly impervious to the electric fire, absolutely prevents discharges from taking place; and that air, being always more or less impregnated with deferent particles, and besides of a very moveable nature, can only retard them. I have many times found a remnant of charge in bottles, sixteen hours after charging them; I found them so, sometimes, after twenty-four hours, or even more; and from my experiments, as well as from those of others, it results, that the duration of such remnants is always in proportion to the dryness and purity of the air: such proportion again proves, that the substance itself of the glass, has not of itself the propriety of transfusing and dissipating charges.

314. The question has been proposed, whether, in order to preserve the charge of a bottle, it must be kept on an insulating or a deferent body. Certainly, if we touch the hook of it, and that the body on which it is laid, be deferent, a discharge will ensue; because the electricity of the hook will dissipate itself through the person who touches it, and at the very same instant the other electricity will also dissipate itself, through the deferent body, into the ground: but in order to resolve the question, whether the electricity be better preserved by placing the bottle so that either, or both, its surfaces be insulated, I have made the following experiment. I hold the belly of the two bottles, A B, *ab* (Pl. II. fig. 6.) and present at once their hooks to the chain, and when the electroscope informs me it is proper time to do it, I take them off

off at once. They being thus charged, I place the one on the ground, the other on the opening of a tumbler or glass, which is very dry, and likewise placed on the ground: and after whatever intervals of time I take up these two bottles, whether it be of one, two or more minutes, I cannot perceive the least spark take place between their hooks, which I all the while present to each other in the dark. Therefore, I conclude that these bottles which are equal and similar to each other, have both of them a long neck (which last circumstance must be observed), and are equally charged, lose equal portions of their electricities in equal intervals of time, though the one be insulated in both its surfaces, and the other in that surface only on which the electricity has been immediately raised.

315. Let us now examine the act itself of the discharge, and the circumstances that contribute to weaken it, the quantity of charge being supposed to be known. Such weakening of the discharge may happen in two different ways; either because the issuing of the spark is *lengthened*, or part of the same is left behind; and the discharge is the greatest possible, only when the rushing of the spark is both *simultaneous*, and consists of the greatest portion possible of the excessive fire. These circumstances may be obstructed by all the several kinds of resistance which the fire may meet with, either in gathering itself from various points of the redundant surface towards that place to which the conducting bow is presented; or in getting into the conducting bow itself, or also in passing through the same; or even in diffusing itself from the same, to the different points on the deficient surface of the glass which is to receive it.

316. Therefore, coatings deferent to a high degree, as I observed before, perfectly continuous, and of a sufficient thickness, will cause the fire most readily to run to that place whence the discharge is excited, and most rapidly to diffuse itself over the deficient surface. I touch several conductors with my finger, which I instantly remove: if the conductor be perfectly deferent, for instance of brass, no farther spark can be drawn from it; but if the same is made with gilded pasteboard, I can then draw a second small spark from it, which is proportioned to the very small, it is true, but yet very frequent interruptions in the gilding. If the

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said conductor be made of pasteboard alone, I then can draw a number of sparks proportioned to the much greater resistance of the naked pasteboard, however damp it may be. I repeat it therefore, that metallic coatings perfectly continuous, will contribute much to unite the discharge, will lessen any remnant of it, and consequently strengthen it; I therefore prefer coating my glasses with sheets of tin or lead, rather than to use water, which is deferent in a less degree; and I prefer such continuous coatings to the use of small shot of lead, or even filings of iron, because the latter do not form so continuous a body: iron filings are besides liable to grow rusty. When the bottles are but small, I fill them with mercury, which of itself is extremely deferent, and touches in all parts the surface of the glass.

317. In the second place, besides, were we minutely to descend into every particular, we might add, that the spark will probably be still more united and simultaneous, if the heads of the conducting-bow are applied to the centre of the coatings: we shall see with respect to this, that a certain *law* obtains concerning the direction of the spark, which will confirm this suspicion; the only consideration we shall offer here to the reader is that of the following mechanic principle, viz. that an elastic fluid will best gather itself into that point, to which it may run from equal distances, on all sides.

318. Certainly, a sufficient capacity and continuity in the conducting bow must also contribute to facilitate the uniting together the whole charge. Thus a very strong discharge will indeed be effected through a strip of gilded paper two or three inches long; but if the same be extended to the length of some feet, the small resistances from the small interruptions in it will be multiplied; the discharge will produce but a small crack, and will only be completed, for a great part of it, by a continued rattling. Commonly, small Chains are used to carry the discharge, and as every ring of it only touches that next to it in a few points, thence results a degree of resistance proportioned to the smallness of those points. The number of such resistances is indeed infinitely less than these from the interruptions in the gilt paper; no great difference, when a chain is used, will result either in the crack, or the other effects of the spark, at least as far as the bluntness of our senses will

will allow us to judge; but yet, such chain as we shall better see hereafter, affords some degree of resistance, therefore I prefer a continued rod of brass, a fourth of an inch thick, and I observe not to anoint with oil the screws into which terminate the two ends of it, which are destined to be inserted into the balls M O; because such ointment would produce some resistance; and indeed I have sometimes observed a portion of the spark externally to lighten from that line which parts the rod from the ball.

319. But a farther resistance can also arise from the conducting bow itself, whatever may be its capacity and intrinsic *deferent* quality; with regard to this, a more minute discussion is, I think, necessary. The experiment of the bottle of Leyden was scarcely known, when the Abbé Nollet and Mr. Monnier, junior, undertook to discharge such a bottle, through long conductors, either to try to what distance such discharge might be carried, or to discover the velocity of its passage. I suspect, that in that experiment especially in which they attempted to discharge the bottle through an iron wire a league long, the discharge took a much shorter way, since the wire lay on the green grass, went over wooden fences, and was afterwards spread over a newly tilled ground. We have observed before, that in order to complete a discharge, there is no necessity that the very same fire which runs from the redundant part, should itself reach the defective surface; we shall soon have occasion to observe, that electrical fire diffuses itself into that receptacle which lie nearest at hand, though it may be of a nature somewhat less deferent, and that it also runs from the nearest place that can supply it.

320. In the course of their experiment, the English philosophers made the same observation, and obviated the error to which they would else have been exposed, by insulating their long iron wire on sticks of wood dried in the oven. I shall relate the last and greatest experiment that was made relative to that object under the direction of Mr. Watson. They insulated an iron wire, which, by the numbers of its windings, completed the space of two miles; the electric Machine was in the middle of the circuit, and there an observator held in each of his hands one of the two wires, each a mile long; the other end of one of the said wires communicated with the outside of the bottle, and the other end of the other wire communicated with the Chain, to which the bottle was suspended. The discharge of

the bottle was repeated several times, and not the least difference in point of time could be perceived between the explosion and shock, by the Observer who united in both his hands the two wires, each a mile long.

321. But what is more essential to our present object, is, that in all the explosions thus made through conductors of a very considerable length, it was observed, that though the bottle was completely charged, yet the crack from the gun-barrel that served as a prime conductor, was less strong than when the discharge was effected in a room; so that, says Dr. Watson, an observer conversant with the subject would not have imagined, from the small spark and noise that took place, that the stroke at the extremity of the wire was in any degree considerable. (See Priestley, p. 207.)

322. Before I had read such observation, I had already perceived something like it in making an experiment which we have occasion to repeat several times in the year in the rooms where our University's machines are kept. In order to make this experiment I pound some gunpowder into a very fine powder or dust, and ram it into a strong tube of glass, so as to fill up all interstices within it; this done, I fix into it two brass rods A B, (Pl. VI. fig. 9.) sharpened at their ends, and place them at such a distance from one another, that the spark may leap from the one into the other; I afterwards place the tube thus disposed, within the circle of the discharge of my ordinary electric square, the coating of which is not quite three square feet, and then I observe the following particulars. I. When the circle of the discharge is short, the crack and the light produced by the spark, are extremely intense, and the latter kindles the gunpowder, as it passes from one of the above points to the other, though distant from each other a quarter of an inch and more. II. But if I make the circle or compass of the discharge much longer (I seldom make it longer than one hundred feet) then, though I may insulate either, or both, of the brass wires that constitute the communication, no kindling of the gunpowder will ensue. III. In order to kindle, it is necessary that the two points should be advanced to an eighth part of an inch from each other. At the same time, the spark does not leap from the electric square but later, that is to say, only when
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that end of the wire which is to draw it, is presented to the coating at a distance less than usual; it is thrown also with less force, and with a much less crack; the light as well as the crack are not simultaneous, but have in them a kind of *succession*, which our senses may, with some degree of attention, distinguish, though the time cannot, I think, admit any kind of mensuration.

323. This experiment serves to confute that dangerous paradox, viz. that a spark most freely circulates through different bodies, without suffering any obstruction from their extent, whatever it may be. It also demonstrates that metals, in whatever degree they may be more deferent than other bodies, yet afford some resistance to electric sparks, which resistance is proportioned to the length of the passage, which those sparks are to take through them, (such resistance in my brass wire could not proceed from its want of capacity, it being somewhat above a twelfth of an inch in diameter) that is to say, proportioned to the length of the column or body of electric fire which lies diffused within the metal, and which the spark, when it gets into the latter, must drive forwards: just as Air, when I blow within a tube, proceeds no otherwise but by driving forwards the air formerly contained in the same. Therefore, the motion of the electric fire is subject to the common mechanick laws of *action* and *re-action*, from which it seemed at first most extraordinarily to deviate. And as a last consequence, I shall observe, that the above experiment, and that of the light we have called *overflowing light* (142.), reciprocally assist and explain each other, as well as confirm the theory.

324. From these facts an important truth moreover follows, which is, that the deference in bodies must not, for the future, be estimated only from the degree to which they of themselves possess such property, and from their electric *capacity*; but must also be looked upon as being proportioned to the length of those bodies, inversely. Certainly, those brass wires which I used, being considered in themselves, are better conductors than my own body, yet, when I join them together into a single wire, so that they constitute a conducting bow two hundred feet long, and try, with such bow, to affect a discharge, part of the fire leaves the bow or wires, and fairly runs through my body, in consequence of the

greater resistance than usual it meets with in moving through that ample and most deferent, it is true, but also very extensive metallic wire.

325. From the above observations, I have farther understood the reason of a fact, which indeed ought originally to have suggested to me this present theory of the *resistance* proportioned to the *length* of the passage, but which I had postponed to mention, till I had a few others more decisive to confirm it. After charging small plates of glass, I sometimes happen to discharge them by touching the coating of the one surface with my thumb, and the coating of the other with the index of the same hand; and in this case I find, that my two fingers are struck much more strongly than when I discharge the same plate by touching the two coatings with my two hands; I shall moreover add this, that in the former case, the spark is also stronger and more simultaneous in proportion with the shorter way it has to go through.

326. In short, we have this demonstrated, viz. that all other things being equal, the shortest passage procures the greatest discharge, but several accidental circumstances in the body that affords such passage contributes also either to weaken or strengthen the discharge. Among such circumstances we must not certainly forget to mention the shape and dimensions of the two ends of the conducting bow, which is to form the communication. If such ends be sharpened, the discharge, if considerable, will not indeed be effected silently, notwithstanding it sometimes happens, that the copious fire contained in clouds, is silently drawn by sharp metallic rods; no, this will not be the case here, unless the end of the conducting bow be presented very slowly, and if the charge be extremely weak, a spark sufficiently copious will indeed be thrown; but if such spark be attentively examined, it will be found to be much divided, the sharp point beginning to draw fire before it reaches the usual distance, and the discharge will in consequence be less strong than when the spark is drawn by a bow terminated at both its ends by balls exactly round, which can draw the fire at once. With respect to this however, some limitations must be observed, because, if the diameter of the balls be too great, another kind of division in the spark will take place, which will equally weaken the effect of it: if, for instance, a given spark, which, in leaping
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into the ball of my conducting bow, which is of half an inch in diameter, keeps united into a space of one twelfth of an inch, leaping now into a ball of a diameter double to that of the former, it will perhaps spread itself on its surface over a space double to that occupied in the first case; all which will be more fully proved in the fourth Section, where I shall speak more explicitly of the effects and accidents relative to sparks.

327. But lastly, the *resisting* medium itself, through which a portion of the spark is to rush, may be so prepared beforehand, as to transmit it more united, more loud, and consequently more effectual. I. Thus I excite the spark through a hole a twelfth or a sixth of an inch wide, bored in a small plate of glass, or of talc; in fact, every body may have observed, that when he presents his finger to the Chain, or to the Machine, when animated by a strong electricity, the fire begins to rattle at a greater distance than that at which sparks are thrown; and thus the substance destined to form the spark is divided, and the latter of course weakened. Now, a circumstance exactly like this takes place, when the conducting bow is presented to the coating of a charged glass, especially if such coating be not even and polished; in such a case, a portion of the charge, if somewhat intense, runs before the rest, partly silently, partly with cracks, and that spark which follows is proportionally weakened; but if the coating be guarded by an insulating plate, the fire that gathers at the small hole remains united, counteracts and balances itself, as we shall see in the next Section, and rushes to the conducting-bow, much more compact and rapid.

328. But I moreover use another contrivance in order still more to unite the passage of the spark into the same point of time; this contrivance is deduced from my principle, *that the spark drives deferent bodies into its own way*, as well as from the experiments I made in the sixth chapter of the *Artificial Electricity*, where I proved that sparks drive water into a most active vapour. I reasoned thus: a few particles of water driven into vapour will occupy a space many thousand times greater than before, and will consequently most rapidly exclude the air from the same space; therefore, if the principle of the spark (we shall see hereafter what is to be understood by the *principle of the spark*) really drives water into vapour, and
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if, according to the above law, it drives it into that way which it marks out for the spark; it will thus open a very free passage to the body itself of it, which is to follow, so that the whole fire will rush through such passage most compact and rapid. In consequence therefore, of such reasoning, I wet a little, either with spittle or water, that portion of the glass which lies around the hole through which I propose to draw the spark, and thence always follows a most loud crack. In short, I help art with the same means which, as I said in another place, (Terrestrial Atmos. Elec. pag. 250.) Nature herself makes use of when she discharges her loud, formidable arrows.

329. Thus, as in the preceding chapter, we have learned how to produce the most intense charges, so in this we have discovered how to effect the most intense and effectual discharges. To the latter purpose, a conducting bow must be used, in its nature extremely *deferent*, of a sufficient thickness, and of the least length possible. The same must be supplied with knobs at both its ends, well polished, and of a proper size; the spark must be excited through a hole sufficiently wide, bored through an insulating body; and the spark must be as it were assisted by a drop of water, through which it may open a free passage to itself, across the resisting medium. Lastly, while the spark is excited, the electricity of the systems must continue to be animated, that any accidental dissipation of the fire may thus be incessantly repaired.

C H A P. VII.

In which some questions are resolved, which serve still more to confirm the theory on the charges and discharges of insulating bodies.

330. **F**IRST question: *Do not the coatings, besides their respectively distributing and uniting charges, somewhat contribute towards increasing the intensity of the same?*

331. Dr. Franklin took off the hook and lid of a charged bottle A, and decanted the water in it, into the bottle B, observing in the mean while to have both bottles insulated; then placing again the hook into the bottle A, he received nevertheless, a shock from it, while he obtained none from the bottle B, into which he had decanted

decanted the water. Perhaps might it be suspected, that the *striking* electricity had remained behind within the fur of moisture which continued to coat the inside of the bottle A; but another experiment made by the same gentleman removes this ground of suspicion; he stripped a charged plate of its metallic coating, and he was nevertheless struck by it. But all objections whatever, on this head, are farther confuted by the experiment I have since made: I charge a naked plate by the means of the brush and star on both its surfaces; and after supplying the same with metallic coatings, which are moreover insulated, I receive a shock: this therefore remains ascertained, viz. that the electricities which constitute the charge, do not reside within the coatings.

332. *The coatings do not increase the intensity of the charges and discharges, but as far as they receive a certain quantity of electricity on that of their surfaces which remains uncovered; which quantity is exceedingly small in comparison to the electricities that reside in the opposite surfaces of the insulating body, which constitute the charge.* In the following chapter I shall prove, that the electricity of *deferent* bodies intirely resides on their surface, and in no shape whatsoever within their substance; but independently of any farther proof we may give hereafter on this head, the long series of sparks that is requisite to discharge an insulating body *by alternation*, (No. 290.) is alone sufficient fully to prove, that each of the sparks drawn in that kind of experiment, expresses the whole electric *capacity* of the coating; whereas it is the sum itself of all those sparks which expresses the quantity of the fire that constitutes the charge.

333. *Nor is such electricity in the coatings, anywise necessary to promote the electricity that constitutes the charges.* Let us take a bottle full of water into which is dipped a sharp point of iron; there is no electricity within the water that serves as an interior coating to the bottle, and when it comes to be presented to the Chain, or to the rubbed glass itself, it immediately takes its charge; yet, the whole capacity of such coating, is reduced to the extremely small capacity of that point of iron.

334. I have covered with sealing-wax ground into very fine powder, the superior surface of the plate of glass A B. (Pl. vi. fig. 8.) and heating the same very slowly, brought it to flow, so

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that a sheet of tin might be fixed to it. I again spread powdered sealing-wax on this coating, and after heating it, I applied to it another plate of glass perforated in G, and also very hot; so that the two plates with the coating thus in the middle, remained very firmly united to each other. I have, proceeding after the same manner, annexed another coating to the other side of the plate A B, and on it another plate of glass E F, perforated in H, so that the whole formed a mass composed of a plate of glass A B, with its two coatings covered with wax in the middle, and of two other plates C D, E F on each side of it. Lastly, with a sharp knife I uncovered all that part of the inward plate A B, which corresponded to the holes G H; I filled the vacancy with shavings of tin, and through such communication I tried to introduce a charge into the inward plate A B. This done, I discharged it; when I received such a shock as I never experienced before from a common plate of an equal size to this. Now, in such apparatus the sealed coatings have of themselves no electric *capacity*, except indeed, that of the very small surfaces of the shavings of tin-sheet, inserted into the holes H, G.

335. Whoever has a sufficient degree of patience, will, by the same means, procure an electric battery most commodious and effectual. Let twelve plates be pasted together in the above manner, with their coatings in the middle between them, and let pieces of the coatings of the plates hang out on each side alternately; then let these twelve plates be again fastened with sealing-wax, between two plates of glass externally naked. In such state of things, the Chain will send fire into those plates which are contiguous to the metallic stripes which get out of the general mass, with which it communicates; and mean while the Machine will draw the fire from those plates which are contiguous to the metallic stripes with which it communicates. The contrary electricities which will take place within the whole, cannot possibly intermix with each other; no electricity besides, will diffuse and lose itself through the margins, since none are left. Lastly, the total weight of this electric battery will be the least possible, if we consider its capacity: it will also occupy the least possible space, since under the volume of less than half a cubic foot, will be comprehended (if each plate admits
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of a coating of one square foot) a battery equal to twelve square feet.

336. Second Question. *To what must be attributed the remnants left by discharges?*

337. *The principal part of such remnants is proportioned to the imperfection of the communication, and is proportioned to it.* When I discharge the electric square A B C D, (Pl. II. fig. 8.) I bring my conducting bow into an immediate contact with the opposite coatings; the greatest possible spark is thus thrown out; and then if I instantly take off the bow, but afterwards touch again the square with it, I only obtain a very faint light; if on the contrary, I do not touch the square, but discharge it by keeping the knob of the bow at the usual distance through which sparks rush, then, when I touch the square again, I draw the remaining fire which has not been able to leap through the resisting medium, air. If I direct the spark through a substance that affords a certain resistance, for instance, though a stripe of tin-foil, extremely narrow, and consequently of a very small capacity, or through metallic *câbles*, which are deferent but in a small degree; in these cases also, when after the discharge is made I touch again the square, I find a remnant of a spark proportioned to such increased resistances; and such a remnant may, by increasing the resistance, be increased at pleasure, and the discharge be diminished accordingly. A distinction however, must be made with regard to these kinds of resistance which as soon as they begin to be surmounted, instantly afford a most free and open passage to the discharge: thus a discharge transmitted through water, which of itself is not a substance deferent to the highest degree, yet leaves a remnant of discharge exceedingly small; the reason of this is, as has been explained before, that water, when driven into vapour, affords a most free passage.

338. But even after a perfect communication has been established between the two opposed surfaces, there is still a remnant of a discharge which is more difficultly drawn, which seems to adhere to the glass, and after successive intervals of time, affords a series of other sparks, whence therefore does such a remnant proceed, which I may call a *reluctant* remnant?

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339. This remnant, I doubt not, proceeds for the greatest part, from the redundant electricity, which, in the act of charging, is driven beyond the coatings, into the naked margins of the glass. Such electricity is perceived, either while it actually accumulates itself on the margins, or in presenting threads to these same margins, which are immediately attracted by them; other circumstances may besides contribute to increase this electricity, such are, I. The strength with which the charge is effected. II. The thinness of the glass. III. The extension of one of the coatings beyond the corresponding limits of the other.

340. In glasses to which their coatings have been pasted with a substance that is neither insulating nor perfectly deferent, it may be, that a part of those *reluctant* remnants proceeds from the resistance which the discharge necessarily meets within such substance. In an electric square to which its coatings were fastened with paste made with starch, the said coatings were, after a matter of a year, for the greatest part separated. I do not know whether this arose from the force of the discharges; but such separation lessened much their intensity, and it has been rendered the same only when I have again fastened the coatings to the plate.

341. In the abovementioned apparatus of a plate of glass coated by the means of sealing-wax, the metallic coatings are immediately applied on the stratum of sealing-wax, which, together with the plate, constitute a single insulating body; therefore, no reluctant remnant can arise from any resistance of the paste that has been used; and that which may arise from an electricity gushing out around the holes above mentioned, may easily be suppressed. I therefore proceeded thus: when I had charged jointly both the waxed plate A B, and another plate of the same size that stood by itself, and was coated after the usual manner, I placed both on a deferent table; I then spread a metallic coating on the outward plate, and the whole mass was thereby discharged. This done, I immediately examined whether there was a remnant of electricity in the coating I had thus placed on the plate C D, by presenting a very thin thread to it, and I always found some signs of electricity, it is true, but they were exceedingly weak, and of an extremely short duration, so that the thread only moved a few times.

times. I then discharged the plate that was coated in the usual way, and examined the state of its coating; when I found it to continue for a very long while to draw the thread with a considerable degree of force; so that the electricity which remained on the coating that had been applied to the former plate, might be called nothing when compared to that in the latter. I made this experiment in winter time.

342. I was therefore by no means induced to conclude, that any considerable part of the *reluctant* remnant proceeds from a quantity of electricity which penetrates into the substance of the plate of glass, to such a depth as to make it difficult to extract it from thence (which was the object I chiefly proposed myself to inquire into); and the more so as even the very small remnant which I perceived in the plate C D, might very well proceed from some imperfection in the contact between it and the metallic sheet, which perhaps I vainly endeavoured perfectly to spread upon it, though I took every possible precaution to make the contact perfect.

343. To conclude; the greatest portion of reluctant remnant in a plate of glass, supplied with coatings exactly shaped and completely deferent, must be attributed to redundant electricities, which by the force of the charge, are driven to a somewhat considerable distance beyond the coatings, and are stronger in proportion as such distance is less. When the charge is suppressed, such electricities gradually dissipate; one part of them gets again into the coatings; the nearest make their way first, and afford the first sparks which are stronger; to these gradually succeed weaker ones.

344. Third Question. *Whence proceed the holes and fissures perceived in glasses, and what kind of information can we draw from them, with regard to charges and discharges?*

345. I blow, (as I observed before in Numb. 468 of *Artif. Elec.*) two thin balls of glass, and fill them with mercury, water, or filings of iron, and insert into their necks one end M of the conducting bow M N O, (Pl. II. fig. 8.) and with the bow thus armed with glass, I try to discharge the square A B C D, when I find that a spark is thrown from it, that bores through the ball, a round hole, which is about one-twelfth part of an inch in diameter;
without

without any other fissure: this is what pretty commonly happens when the balls are made of an excellent glass, without bubbles or other imperfections, and every where equally thin.

346. But in glass less perfect, or even in those that are perfect when the operation is attempted after a different manner, many accidents take place, of which I shall only mention the most remarkable. I. When there are bubbles in the substance of the glass, it seems that the spark marks out and chooses those that will resist least; nor does it always perforate the whole of such bubble, but sometimes only bores a little hole, through which the point of a small pin hardly could pass; sometimes a fresh hole is made in the centre, sometimes in the side, sometimes, where two bubbles join: I commonly discover such holes, by blowing into the ball and receiving the wind on my hand. II. It often happens, that from the hole, whether it be narrow or wide, several fissures run, which frequently unite into one, so that pretty large pieces fall from the bottom of the ball, which makes it difficult to find out the original hole; even sometimes the whole bottom is beaten into the ball. III. At times, the hole happens not to be completely round, and then it is of an oblong or angular form where the circle ceases. This however must be observed, that that hole which is the immediate effect of the spark, has in it very distinct characteristics from those which simply result from accidents in the fissures: the edges of the latter are polished and bright, as is the case in all common fractures of glass; but the edge of the hole made by the spark, is covered with furrows converging towards the inside of the ball; which make it look rough and whitish, as is the case with glass reduced into a fine powder. IV. When I put in the inside of the ball, neither water, nor filings of iron, but simply insert into it the end of the conducting bow terminated into a point, then the furrows on the edge of the hole converge towards a nearer point within the ball, and the hole grows much more contracted from the outside towards the inside of the ball: all which is conformable to the nature of the spark itself, which grows contracted in proportion as the compass on which it is to act, grows narrower.

347. But

347. But of all such holes, those which afford most information are, those which are least perfect, that is to say, those in which the portions of glass rent by the spark, partly or all of them remain in their respective places. It is not a rare case that a number of them remain unmoved; but that all should remain is a very rare thing, and which out of a great number of attempts has only succeeded to me once or twice; I shall give a description of the most remarkable case I have observed. I. Exactly from the same visible point on a ball of glass of a middling quality, (I had put filings of iron into the ball) seven fissures ran, five of which were extended to two-thirds of an inch, from the point itself; the two others went to the distance of an inch and more; and the sector contained between the longer fissures was raised somewhat above the sectors contiguous to it; whence I guessed that the stroke given by the spark had rapidly dilated the glass, and rent it so as to raise the sectors included between the fissures, the largest of which had been prevented from falling back by its own asperities, and by those of the sectors contiguous to it. II. But the most remarkable circumstance was the opaque whitish colour observable on the top of the sector around the point from which the fissures sprung; the place thus rendered opaque, formed a circular band a tenth of an inch broad, of which the said point was as the centre; this band being observed with a microscope appeared as if it were divided into a circle more opaque than the rest, then into another circle a little more transparent, into another surrounding the latter, which was composed of several white points, and into a number of others of a changeable colour. The edge of the fissures within the extent of the white circle was likewise white; but examining it with a lens the focus of which was six lines distant, the whiteness ceased to be continuous, and was divided into a number of shining-coloured particles; the sector that had remained raised above the others afforded me an opportunity of examining its edge.

348. Now, the result of all these observations appears to me to be this; I. The hole could not be produced by a simple mechanical stroke of the spark on the surface of the ball, but all the abovementioned accidents demonstrate both an internal action, and a penetration. II. My opinion is, that in order to produce all the above accidents,

accidents, the spark must have vehemently shaken and driven forwards the electric fire inherent in the substance of the glass, and have followed it with its whole stream, dilating the minutest interstices within the substance of the glass, and dividing its particles so as to leave them adherent to each other only in some few points; whence arose the whitish colour of the glass, exactly like that assumed by it when powdered very fine. III. I think moreover, that in consequence of this penetration and passage of the spark, such a dilatation must have arisen by the force of the penetrating and heating element, as to have broken the glass in the shape of seven rays, and raised the largest sector above the others.

349. Besides the whitish colour of the lucid, or coloured particles, both the whiteness of the fissures which ran between the lucid or coloured parts, and the lucidity or colour of the abovementioned points, prove, that all these accidents arose from mere separations of parts; the one very minute, the other somewhat more continuous. I did not forget to examine whether on that spot where the white colour obtained, there was any sign of a mixture of a matter rather calcinable than vitrescible, and could not find the least indice; the surface was extremely smooth, without the least appearance of any rugosities, and the whitish colour lost itself into the contiguous transparent glass, not at once, but gradually. Lastly, when I again observed the fissures, either by the common light of the day, or with a microscope well illuminated, I found that the white appearances on the exterior edges intirely vanished; they were changed into a number of very sharp bright points, and these edges besides appeared as if composed of thin strata, the one more, the other less, extensively broken.

350. With regard to the circular ring around the lucid points, I shall defer to speak of them till I speak of the similar phenomena, which have also been observed by Dr. Priestley, on the surface of metals; I shall only say here, by the way, that such points proceed from such portions of the spark as are scattered around the central and main body of it.

351. But, to proceed at length to the question which I at first proposed to resolve; from the above observations I strongly conjecture, that *the charges and discharges of glasses are produced by an*
action

action which is propagated from the one surface of the glass to the other, not so much by the means of any vibration of the glass itself, as by the means of a vibration, or commotion, or modification, whatever they may be, of the electric fire inherent in the glass. In fact, if such a vibration or commotion as we mention, leaves behind it those visible marks we have just described, when a very strong charge is communicated at once by *distribution*, (when the balls of glass receive the stroke, they become very strongly charged by *distribution*, as much as the nakedness of the one or both of their surfaces will admit,) it is most likely that a vibration of the same kind, though less, will serve to produce some degree of charge.

352. This would also obviate another difficulty of some importance, which is, how bodies so different from each other as glass, porcelain, sulphur, rosin, &c. take a charge in the same manner: it is enough for this, that the element inherent in them be the same, and possess in all the same aptitude to receive similar vibrations.

353. But when I afterwards attentively considered the experiment which I had already described in Numb. 302 of *Atmos. Terr. Elect.* my suspicion turned into a certainty. The experiment I mean consists in transmitting a strong spark through a deferent body of a scanty capacity, for instance, through a stripe of tinfoil, inclosed between two plates of wax: in the instant of the passage the wax becomes as it were transparent, and is really seen internally to lighten.

354. This experiment appeared to me interesting and worthy of being repeated: in order therefore to ascertain the fact, that the light did not barely shine through the wax, but really extended itself through its substance, I inserted two small brass rods into a thick wax candle, so as to make them meet in the middle; then sending the spark through them, I saw the candle filled, in that part which surrounded the place of the contact, with a light that grew gradually less in proportion as the distance from that place became greater.

355. In order to enquire whether such quality was peculiar to insulating bodies, I afterwards repeated the same experiment with candles of tallow, of pitch, of cement, of sulphur, of colophony, of sealing-wax, and lastly with one that was made with a mixture

of colophony, sulphur, and rosin. I also tried the experiment by applying the two points to the surface of a piece of porcelain, and fastened them upon it with a thick stratum of wax; I experimented in the same manner with a piece of opaque white glass; and always in its passage the spark diffused a most ample vivid light throughout the above mentioned substances, which however exhibited different colours within them: and those substances, which were of a somewhat brittle nature, such as cement, sulphur, &c. had been previously warmed a little, lest they should break.

356. Who could therefore help thinking, that those lights we mention, proceed from a vibration of the electric fire particularly inherent in such bodies? I indeed confess, that I never was able to see any light shine within the substance of a china cup, in the act of charging it: the sparks that ran from the outside of it into my hand did not enable me to perceive the black iron-filings that I had put into it; but this does not however destroy the hypothesis. It might very well be that the reaction of the sparks rushing out of the outside of the cup, being equal to the action of those that were accumulating on the inside, did not allow to the fire inherent within the substance itself, a sufficient vibration; no visible lightning therefore could take place. At the same time, the short vibrations of that same fire transmitted to the external fire the motion introduced into the internal one, in the same manner as in a series of elastic balls, a shock from the ball A will drive only the last one F, and leave all the intermediate ones B, C, D, E, in their respective places.

357. But such a comparison drawn from the mechanic shock of balls, would not entirely account for all the phenomena of the charges and discharges of glasses. It would, at most, explain how the excessive fire can accumulate itself on the one surface, when it becomes enabled to drive the natural fire from the other; and how it happens that this same fire, as it were, runs back, and never accumulates itself when the opposite surface comes to be insulated; which is owing to the series of parts that are to be vibrated, being then, as it were, infinite. The same comparison would perhaps also explain how the fire drawn from the one surface, cannot be restored to it, but inasmuch as the excessive fire accumulated on the
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other

other surface can be driven from it: but it would not explain the two following very material circumstances, viz. how it comes to pass, that the natural fire cannot be drawn, but as far as a quantity of excessive fire can run to the opposite surface; and how the excessive fire accumulated on the one surface, cannot be dissipated, but when the natural one can return to the other.

358. In order to satisfy the reader with respect to all the above mentioned circumstances, we might perhaps have recourse to a certain manner in the position, or adhesion, of the particles of the electric fire in the glass, either with each other, or with the glass itself. But where is the system in nature, about which we are not to entertain doubts, if we undertake to measure its probability from the extent of our comprehension? The connection between the two opposite electricities is evident from all the experiments and observations that have hitherto been made; our inability completely to evolve and explain such connection, should persuade us of the shortness of our views, but by no means induce us to deny what we see, or hurry us into fictions which at first pretty well agree with the facts; but afterwards, when attentively considered, are found to be totally repugnant to them. This precipitation we mention, seems to be the case with certain persons, who, too sensible of the difficulty of explaining the cause of the two contrary electricities, raise a sort of rumour against the Franklinian Theory, and choose to explain the above facts, by the existence of two distinct fluids; but, for all their endeavours, they cannot make us believe that two fluids run from each other several miles, in consequence of their tendency to be more closely united together.

359. But perhaps I have dwelt on this subject more than was necessary: let us return to facts. I shall conclude this article with relating the experiment with which in the N. 168 of *Terr. Atmosf. Elec.* I proved the equality of the actions exerted on the two surfaces of a plate of glass by the excessive fire when it runs from the one, and the natural fire when it returns to the other: we shall derive thence more information than from any hypothetical disquisition in which we might engage. This is the experiment: I fasten with sealing-wax the extremities of the two silk strings A, P, (Pl. VI. fig. 11.) which are six feet long, to a coated glass

plate, A B, and I suspend the same by these two strings : so that it freely hangs in a vertical plan. II. Near the centre of oscillation, I annex to both coatings two half-balls of brass, C D, which exactly correspond to each other. III. Two insulated men, the one communicating with the Machine, also insulated, and the other with the Chain, touch the opposed surfaces of the plate, each with a stripe of tinfoil, for fear of agitating it, and it thus takes its charge. IV. This done, I place myself in such a situation that I may see at the same time the two half-balls, and present to both at once the two heads of a conducting bow *; then the discharge is immediately completed, and not the least agitation perceived in the plate.

360. I make the same experiment with still more exactness, though with an apparatus somewhat more complicated. (Pl. VI. fig. 11.) I insulate, on two little columns of glass, a brass rule, L N, from which hang two metallic pendulums, E F, G H, terminated into two balls of brass. I then place the brass rule over the plate, so that the two pendulums hang on each side of it, and almost touch the half-balls C D; they are only prevented from actually touching them by two threads, F I, H K, by which they are tied from behind to the two little columns of glass. Things being thus disposed, the charge is begun, and in the mean while the two pendulums are raised and kept close to the brass rule; when the charge is completed, I let fall both pendulums at once, and they at the same instant reach the two half-balls, at least as near as they are allowed by the two threads that are to stop them; thus the discharge is effected, and the plate remains unmoved.

361. Fourth Question. *Whence the fracture of the glasses observed by Dr. Priestley?*

Dr. Priestley made an experiment with a battery of one and forty glass jars, having each a coating a foot square, and often in the act of the discharge, one, two, or more jars happened to break;

* Instead of my usual conducting bow, I use, on this occasion, a brass wire, or string of a harpsichord, that I may easily bring the two knobs together; and instead of the usual knobs, or brass balls, which terminate my conducting bow, I use, on account of their lightness, two of those so very light and thin golden balls which our country women call *dorini*, and use to wear in their necklaces, or trinkets.

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sometimes in several places. In a battery of thirty-six jars, having a coating a square foot each, I often happened to find four of them broke, always one at a time; but for three different times I have only perceived the breaking of the glass several days after using the battery; whence I may doubt whether such breakings were not merely accidental, like those of glasses not sufficiently heated again in the furnace, and in some places too thin. I have seen only once the corner of a jar lighten while I charged it for the second time in the same hour, and found it broke; but I could discover nothing more than what had been seen by Dr. Priestley. Moreover, one day as I was watching for the moment when the bottle, A B (Pl. II. fig. 5.) should cease to give sparks, I saw a spark thrown from E, where the handle was fastened to the body of the bottle, and the disjointed handle remained in my hand: I was quick in catching the bottle with my other hand, and only perceived that the handle had been imperfectly fastened; and could draw no kind of information from all these fractures in my glasses; I only remarked, that after stopping with sealing-wax the hole made in the bottle, it could, as it did before, receive any degree of charge.

363. But though I have been unable to analyse by experiments these accidents in the glass, yet I shall hazard some conjectures with respect to them. I. I suspect that the discharge of the jars that composed the battery of Dr. Priestley was not really discharged at once; since we have seen that a discharge, when it has a great space to run through, meets with some degree of resistance, and grows divided. II. Therefore I suspect that the excessive fire introduced into those jars which are nearer the places through which the communication is formed, rushing at first very compact and dense into the knob of the conducting bow, is repercussed backwards, and encreases in some degree the density of the excessive fire that is retarded in some other jar: to such sudden repercussion I think we must impute the different fractures of the glasses.

364. It seems to me that this hypothesis would account for the various accidents observed by Dr. Priestley with regard to the breaking of glasses. I. It would explain their multiplicity: it is very natural

natural to think that a stroke from the fire contained in thirty-five jars, against that contained in six (this is the greatest number that has broke with Dr. Priestley) may exert itself at once on all their weakest parts. II. It would also explain why such fractures obtained, whether he effected the discharge himself, or whether it spontaneously took place. III. The same hypothesis would also explain the most surprising accident of all, which is, that though some jars were broken, the effects of the discharges on the body through which the communications were established, such as melting and dissipating iron wires, were equal to those observed when all the jars remained whole. The reason of this is, I think, that in the discharge of such a battery the *momentum* of the discharge is constituted by that portion of fire which, immediately and at first, rushes and forms the spark; and the excessive fire repercuſſed from a few jars, and kept back, is only that which serves, not to render the body of the spark more effectual, but to form a kind of *trailing* after it, which trailing is so conspicuous in lightning. But to repeat here what I said before, I here only mean to offer my conjectures on this subject, which I propose to continue to investigate by observations and experiments; and I shall always be ready to embrace any opinion, well supported by facts, that may be proposed to me.

C H A P. VIII.

Of the Charges and Discharges produced by Friction.

365. **T**HE excitation of the electricity by the means of a friction is an object, till now, as dark as it is important. I think I have, in the appendix to my first chapter, explained clearly enough the reason why, of two bodies that reciprocally perform a friction on each other, the one, rather than the other, imparts its fire. Now I propose to enquire what accidents take place in an insulating body that *receives* the electric fire, for instance, from the hand; whence we may also conclude what must be the case with an insulating body, whose nature is to *give* it, for instance, the sealing-wax, which gives its fire to the hand.

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366. And first of all, it appears from experience, that *it is not necessary, in order to render an insulating body positively electrified by friction, that a contrary electricity should arise in the opposed surface to that where the former is produced.* In fact, an electricity by excess is very well excited by friction on a solid thick piece of glass; an electricity by deficiency, on an oiled stick, or a stick of sealing-wax. Now, the manner in which I excite those electricities, viz. by rubbing only the edges of the plate A B, (Pl. I. fig. 10.) shews that the rise of a contrary electricity on the surface opposed to that which is rubbed, is so little necessary, that an electricity of the same nature may very well be excited on both surfaces of an insulating body, at the same time, if they be rubbed jointly.

367. It results also from experiments, that *when only one surface of a piece of glass is rubbed, no electricity ever takes place on the opposed surface, but after the rubbed part has moved from the hand or body that performs the friction. Then, and then only, the excessive fire deposited by the hand in the act of the friction, displays itself on that part which has just been rubbed; the one part of which fire runs back into the next extremity of the rubbing body, and then forms what we called above, the departing light; the other part stops and accumulates itself on the glass, and then vibrates, and drives the natural fire inherent in the opposite surface.* The following is, among many others, the simplest experiment that demonstrates the above proposition. Let the circle *a b c d* (Pl. VI. fig. 12.) express the plate of which I commonly make use instead of a globe or cylinder; and S, one of the rubbing cushions, the other being taken off in this experiment. I present a flax thread exactly to that part of the glass, which, with its opposite surface, presses upon the rubbing cushion; meanwhile another person makes the plate turn forwards in the direction *a b c d*, and no motion takes place in the thread near the place in contact with the cushion, but the same is obliquely attracted towards *a*, that is towards that part of the glass which has just left the rubbing body: I repeat the experiment several times, and the result proves constantly the same.

We must therefore conclude this, viz. *that the excessive fire deposited on the glass by the rubbing body, is not able, in that very instant when it is deposited, to vibrate the natural fire from the opposite surface:*

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the friction only seems to increase the natural electric capacity of the glass in that part which is actually rubbed; so that the fire added to it through the friction, becomes as it were natural to it, and nowise redundant; and therefore makes no effort against the natural fire of the opposite surface.

368. Hence it happens that the friction introduces no beginning of a charge, except where the rubbed part begins to escape from the contact. In that part alone the rubbed surface resumes its natural electric capacity, so that an accumulated fire then begins to display itself, and endeavours to introduce a deficiency on the opposite surface, by vibrating and driving the natural fire from it. The truth of these principles may be rendered conspicuous by experimenting in a dark place, and presenting a point to the above mentioned plate, (Pl. VI. fig. 12.) and making the same turn with a sufficient degree of rapidity. So long as the point is directed to the rubbing-cushion, no light appears on the plate. In order to have the little star shine on the point, the latter must be presented a little beyond the cushion: nor must it be presented precisely to that part where, at the parting of the plate from the hand, the *departing* light appears, but a little farther: for it seems that the fire requires some small space of time to be able to manifest itself, and drive the natural fire from the opposite surface.

369. We have therefore analytically discovered here a new and wonderful way of charging glass: the same man may, with one of his hands, give extensive fire to the one surface of the plate, and, with the other hand, take the natural fire from the other surface of it: the same man can perform the two functions of both an animated system, and an indifferent system!—I take from its place the rubbing-cushion S, (Pl. VI. fig. 12.) I rub the plate with the palm of my hand, and with my other hand I present a very sharp point to the opposed side of the plate, in *a*, a little beyond the *parting* light, and I order the same to be turned evenly. At first a very vivid spark shines on the point; then it grows faint by degrees; the *parting* light, on the contrary, grows continually more copious, and at last shews that the charge is completed. I then remove, at the same instant, both my hand and the point, and order the motion of the plate to be stopped: then I take a brass rod, one line thick,

thick, rounded at its end; and, at the same time that I successively present my hand to the different parts of the rubbed surface, I present the brass rod quite close to those parts of the other surface which correspond to them, when vivid brushes spring from every part with a rattling noise; and thus I complete the discharge by the means of a number of successive brushes. I may also silently effect the same discharge, by applying both my hands to the two opposite surfaces: I can also effect it with a stroke, viz. in coating the two surfaces, one at a time, and then touching them both at once.

370. And this method of producing a charge by friction, however surprising it may appear, as it is effected by a single person, or to express it in more universal terms, by the same conductor, yet is so clear, that it scarcely wants an explanation. I. The friction from the hand increases the natural electric capacity of the glass; therefore, a quantity of fire flows from the hand into the glass. II. That part of the glass which escapes from the friction, resumes its lesser capacity; the deposited fire then manifests itself on it, and drives the natural fire from the opposite surface into the brass rod which is presented to it. III. When, afterwards, the glass by turning gets again under the rubbing hand, the friction increases a-new the natural capacity of the rubbed surface, as far as the quantity of natural fire which the opposite surface has before lost will allow it, therefore, deposits some additional fire upon it; till at last an equilibrium is produced between the force of the friction for increasing the electric capacity of the rubbed surface, and the resistance from the other surface against sending any more of its own fire.

371. The discharge is likewise very easily understood; in order to effect it, it is enough to present the point to the inside of the tube, opposite to that part which is rubbed. I said before, that when I present the thread, or the brass rod, opposite to my hand, or any other body that performs the friction, the rod draws no fires and the thread is only obliquely attracted towards that part of the glass which has just escaped from the friction. But this, we must observe, only obtains in the beginning of the operation; for if the charge has been farther promoted after the manner we have just

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described;

described ; then, the thread is directly attracted ; and then, if we present the point opposite to the rubbed place, a very vivid brush rushes from the point, which destroys the charge.

372. This experiment confirms two truths, and points out a third. I. It confirms this truth, *that the excessive fire deposited by the hand on the rubbed surface, diffuses itself into that surface without making the least effort (during the immediate act of the friction) to drive the fire from the opposite surface ; but diffuses itself into the glass in consequence of, and as due to, the then greater capacity of it.* II. It confirms this second truth, *viz. that it is only beyond that part where the friction is performed, that the deposited fire manifests itself ; that is, at that place at which the glass returns to its lesser natural capacity ; and it is likewise at this same place, that the natural fire begins to be driven from the opposite surface.* III. The same universally demonstrates, *that that part of the surface of the glass which is opposed to that part of the other surface which is rubbed, is exactly in the same disposition as to receiving back the natural fire of which it has just been deprived, or an additional fire, or again to throw its own fire, as if the other surface were not rubbed.*

373. These truths prepare the way for producing and analysing another method of forming a charge by friction ; I mean to speak of that kind of charge which is effected by rubbing the naked surface of a glass the other of which surface is coated. The experiment has been first invented by Dr. Franklin, who, by his vast and penetrating genius, has, without any farther assistance, found the hypothesis of the *natural electric capacity* of the glass being increased by the friction. Place, says he, at p. 80, a wire into the tube, “ the inward end in contact with the non-electric lining, so it “ will represent the Leyden bottle. Let a second person touch “ the wire while you rub, and the fire driven out of the inward “ surface when you give the stroke, will pass through him into “ the common mass, and return through him when the inner surface resumes its quantity, and therefore this new kind of Leyden bottle cannot be so charged. But thus it may ; after every “ stroke, before you pass your hand up to make another, let the “ second person apply his finger to the wire, take the spark, and “ then withdraw his finger, and so on, till he has drawn a “ number

“ number of sparks ; thus will the inner surface be exhausted, and
 “ the outer surface charged. Then wrap a sheet of gilt paper close
 “ round the outer surface, and grasping it in your hand, you may
 “ receive a shock, by applying the finger of the other hand to the
 “ wire ; for now the vacant pores in the inner surface resume
 “ their quantity, and the overcharged pores in the outer surface
 “ discharge their overplus.”

374. This explanation does not, however, comprehend all the admirable accidents I observe in such charge. I line one half part of the inside of the tube BA (Pl. IV. fig. 3.) three feet long, and one inch and an half in diameter ; I make a few inches of the lining hang out of the tube in BC, that I may annex two threads to it ; I observe to grasp the tube by its unlined part DA ; and rub it from B towards D ; keeping my hand at a distance from the tube, when I bring it back to B, in order to begin again the friction.

375. And I observe, I. That in the act of friction, especially at the first, no electricity is manifested either from the outer, or the inner surface of the tube ; a thread hanging close to the outer surface, does not move in the least, nor do the threads in C diverge ; but when I suddenly cease to rub, and remove my hand from D, then the threads in C diverge, and that annexed to the outer surface of the tube is attracted.

376. II. I observe, that the charge is so formed, that it is greatest near B, where I begin the friction, and from thence towards D, it gradually lessens. In order to ascertain this, I do not coat the outside of the tube ; for in this case I would only obtain one general discharge, from which I could perceive no difference between the several charges of which it is made up ; but I successively grasp different parts of the naked tube, when I obtain several discharges, which become gradually less, as they are successively drawn from a part nearer to D.

377. Now, in order to explain these accidents with a sufficient degree of clearness, I consider the outer surface of the tube BD, as if it were divided into small and equal circles, A, B, C, D, E, F, . . . Y, Z ; and I suppose the inner surface to be likewise divided into corresponding circles, *a, b, c, d, e, f . . . y, z*. Then I say, all the fire, which, in consequence of the just performed friction, is vibrat-

ed from the precedent circles *a, b, c, d*, immediately runs through the lining into the nearest circles *e, f, g, h*, which the rubbing hand at that very time grasps; so that when the hand, at the conclusion of the friction, becomes applied to the circles *V, X, Y, Z*, the whole fire that has been driven from the preceding circles, is affixed to the circles *u, x, y, z*. And thence may be understood how it comes to pass, that, in the act itself of friction, the threads in *C* do not diverge, and that annexed to the outer surface is nowise attracted. At the same time, that the circles *A, B, C, D*, escape from the friction, the fire that has been deposited on them, and which, if the tube were not lined, would manifest itself by visible signs, is only *accumulated* and the natural fire which it has been able to drive from the inner circles *a, b, c, d*, through the lining, goes through that same lining to accumulate itself on those of the following circles, that correspond to the rubbing hand, and introduce there a *consequent* charge; therefore, the excessive fire, since it is accumulated and forms a charge, does not attract the thread annexed to the outer surface of the tube; and, for the like reason, the natural fire accumulated on those inner circles to which the hand is applied, does not attract the threads in *C*.

378. I say, in the second place, that the fire deposited by the rubbing hand on the last circles, decreases as the quantity of the fire deposited on the former circles increases: the reason of this is, that the sum of the fire which from the inner circles *a, b, c*, runs to accumulate itself on those which are nearest to them, is equal to the sum of the fire accumulated on the outer circles *A, B, C*. Now the fire which is thus accumulated on the inner circles *d, e, f*, endeavours to introduce there a consequent charge, endeavours to drive the natural fire from the outer surface, and the fire which the rubbing hand is to accumulate on that place in the act of friction, must needs meet with a kind of opposition, and does little more than preserve the same place in its natural state; therefore, in proportion as a greater quantity of fire will have flowed from the first circles *a, b, c*, to the succeeding circles, *d, e, f*; the quantity of fire which the hand will be able to leave on the outer circles *D, E, F*, will be less. Whence it necessarily follows, that the charge in the succeeding circles will grow gradually less, and will follow in that respect a certain proportion.

379. Let

379. Let us suppose, that the hand being first applied to the four circles A, B, C, D, leaves the circle A, which has already been rubbed, and now reaches the circle E; let, moreover, the quantity of fire which the rubbing hand has deposited in A, be called 1; from the inner circle *a*, the driven fire will pass into the circles *b, c, d, e*; and lessen, by a fourth part, both the fire which the rubbing hand is to drive from them, and that which it is to accumulate on the outer circles B, C, D, E.

Let now the hand leave B, and reach F; there will be in *c, d, e, f*, both the former fire proceeding from *a*, and the three-fourth parts proceeding from B; that is, seven-fourth parts in all, which, distributed into the said circles, will lessen for $\frac{7}{16}$ the fire which the hand is to drive from *c*, and to deposit on C; so that it will only deposit $\frac{9}{16}$.

Let the hand leave C, and occupy A; there will be accumulated on the circles *d, e, f, g*, both the fire from *a*, the three-fourths from *b*, and the $\frac{9}{16}$ from *c*, which will make up $\frac{37}{16}$, and this quantity being distributed into those four circles, will diminish for $\frac{37}{64}$ the fire which the hand would deposit in D; therefore, it will deposit only $\frac{27}{64}$; so that writing under the circles.

A, B, C, D, E, F, G, &c.

the series 1, $\frac{3}{4}$, $\frac{9}{16}$, $\frac{27}{64}$, $\frac{87}{256}$, $\frac{243}{1024}$, $\frac{729}{4096}$, &c. we shall see disposed in a regular order, though somewhat inexactly indicated, the proportion which the decrease of the charge follows in the different successive circles; whence we shall perceive, that the quantity of fire introduced into the latter circles never reaches to an equality with the quantity of fire which the hand would deposit on them through the effect of the friction.

380. However, the above series only expresses the quantity of fire deposited by the first friction; the quantities that the hand will deposit by succeeding frictions on the last circles, will gradually increase in proportion as the sum of the fire already driven from the first ones, and extracted by the means of the sparks drawn from C (Pl. IV. fig. 3.) will be greater; and as the fire which, in consequence of the friction of the first circles A, B, C, D, will be driven from the inside ones, *a, b, c, d*, into the successive ones *l, m, n, o*, will thus grow continually less, it is very possible, that by repeating the frictions a great number of times, and
continually.

continually drawing off the sparks, at length a complete charge, and equal throughout the tube, may be obtained.

381. But without losing time in inquiring after what is possible, let us attend to that which is practicable; let us first explain a few accidents attending that method of producing a charge, which accompany those above mentioned, and at first sight appear very strange. After rubbing the tube several times, and drawing off the sparks from C (Pl. IV. fig. 6.) when I again apply my hand near B, in order to rub one stroke more, it happens, I. That the threads in C, which were fallen in consequence of the drawing a spark from C, diverge again, being now electric by deficiency, and the tube appears likewise as if it were electric by deficiency at its extremity; all which, however, is quite conformable to the theory. When I apply my hand to B, I lessen the excess that obtains there; therefore, I lessen the deficiency in the corresponding inner circles, to which a certain quantity of fire can flow through the metallic lining, either from other inner circles, or from that part of the lining which hangs out in C (we shall in the next chapter see that no fire can be extracted from the inward substance of the lining, or be accumulated within the same); hence it becomes necessary, that the threads in C should diverge in consequence of the deficiency introduced into them; and what little fire flows from the inner circles corresponding to D, to the inner circles near B, is also sufficient to make the outer parts of the tube near D appear electric by deficiency.

382. I do not mention this circumstance, that when I do not apply my hand to B, till after rubbing a number of strokes and extracting the sparks from C, the threads in C diverge still more than before; because it is obvious, that as the introduced excess is then greater, it of course lessens, when the tube is touched by the hand, in a greater proportion than in the former case, therefore, the deficiency in the inner corresponding circles likewise lessens more, and a greater quantity of fire runs to them, both from the other inner circles, and from the portion of the lining which hangs out in C.

383. I omit likewise, that when I continue rubbing, after a great number of frictions are already performed, and a number of sparks have been drawn, the thread annexed to the outer surface is somewhat

what attracted by the tube, even during the act of friction, and the threads in C lose their divergence by deficiency, and begin to diverge by excess; so that the greatest attraction of the former, and divergence of the latter, always take place at the instant when I remove my hand. The reason is, that as the introduced charge is become greater, a portion of the fire deposited on the preceding circles, begins to exert itself, though during the act of friction, (in the same manner as we see that a greater portion of fire is sent into the Chain, and manifests itself visibly around it, according as the excess in the cylinder of glass is greater); and this fire which manifests itself on the outer surface, will cause a small portion of that which is driven by the action of the inner circles, in C, to manifest itself.

384. I shall now pass to the explanation of other accidents which are common both to the charge of the above-mentioned tube, and to a few other celebrated experiments. It is well known, that if a cylinder, such as that in Pl. III. fig. 14. be lined, either intirely, so that the lining communicate with the ground, or only in that part of it F, O, E, N, which is rubbed, it will communicate no electricity to the Chain. I annex to the Chain a bunch of metallic threads, or wires, that hang over the cylinder of this fig. 14; they spread themselves over that part which is inwardly lined, and, besides extend themselves over the unlined part towards N. If, by stretching one of my fingers, I rub the unlined part in N, the Chain becomes electrified; if I rub only the zone inwardly lined, no electricity arises in the Chain.

385. This case is like that of the tube which causes no divergence in the thread in C (Pl. IV. fig. 3.) and does not draw the outside thread at the first friction, and never can be charged, unless after every friction a spark is drawn from C; such tube, therefore, exactly represents the above cylinder with an insulated lining ON (Pl. III. fig. 14.) and this cylinder may be charged equally well as the tube, if it be managed after the same manner. Thus, I remove the Chain from the cylinder, then alternately rub it, and alternately insert a rod through a hole I have bored through one of its wooden armatures, and draw a spark from the lining ON. After drawing thus a number of sparks, I take the cylinder from the screws, and place the rod *un* (Pl. III. fig. 7.) through the hole;
in

the mean while another person holds the cylinder by a silk thread ; I outwardly coat it in E F ; and as soon as I touch the rod in *u*, with the head C of the conducting bow C T *e*, there springs in *n* a most conspicuous *brush*, and a *little star* shines in *c*.

386. In the same manner, if the lining of the same tube communicates with the ground, it will exactly represent the cylinder communicating with the ground, and contract no electricity.

387. Now, in all these experiments, either with the tube, or the cylinder, there is this common to all. I. That the fire deposited by the rubbing hand on the rubbed surface, does not manifest itself by outward signs when this surface escapes from the hand, (as is the case with unlined glasses); but accumulates itself within the glass, and there produces a beginning of a charge; the reason is because the natural fire may then from the inward surface flow away through the lining. II. In all the above experiments, that small quantity of natural fire which is driven from the inward surface by the small portion of excessive fire which accumulates on the outside, is always to be found in the lining.

388. The effect produced by the first of these two circumstances is, that the first friction sends no perceivable electricity to the Chain, and from the second circumstance it results, that the following frictions send no more than the first. The first friction sends no electricity to the Chain, because, as the small quantity of fire deposited at first by the rubbing hand, drives an equal quantity from the inward surface, it only introduces a beginning of a charge. To understand this better, grasp a bottle hanging from the Chain, and give a spark to the latter; the electrometer annexed to it will not for all that move, at least visibly; this is because there is the same proportion between that portion of the spark, which, in such case, remains around the Chain and can manifest itself, and that which goes to the bottle, as there is between the electric capacity of the Chain and that of the glass-bottle, which is incomparably larger: now, the same will take place in the case of the cylinder we speak of, when, after the first friction, it will be in contact with the Chain. Nor will subsequent frictions augment such charge; because the small quantity of fire accumulated by the first friction will get back into the hand, when the same part of the glass returns to it; and it will be

be able to do, since the natural fire can, through the lining, return back to the inward surface, and destroy the weak deficiency at first introduced into it; so that the outer surface will return to a second friction, in the same state as it was when it underwent the first; it, therefore, always moves from the rubbing body with but a small portion of fire outwardly accumulated on it.

389. Besides, if the interior lining be insulated, the small quantity of fire that issues from the bands, or zones, *a, b, c, d*, &c. (I consider the outer surface of the cylinder as divided into a series of bands, or zones, A, B, C, D, and the inward one into another, *a, b, c, d*), endeavours correspondently to the bands A, B, C, D, to flow back into the subsequent bands *e, f, g, h*, to which the hand is then outwardly applied, as it happens in the tube above-mentioned; and such fire, after one turn of the cylinder, finding itself wholly accumulated against the place of the friction, will repel all the fire which the hand may seek anew to deposit on the same.

390. This fact, that the electric fire successively runs from those places which are outwardly left by the hand, into the subsequent inward ones to which the hand becomes outwardly applied, is visibly demonstrated by one of the most pleasing electric experiments yet made, and reciprocally affords a conspicuous explanation of that same experiment, which would otherwise be one of the most difficult to understand. This experiment is become too important not to be minutely described. I take the cylinder B A C (Pl. II. fig. 1.) with a brass armature in B D F; the female screw E may be fitted to the male screw that rises from the basin of the air-pump; the small key D serves to preserve the vacuum, the screw F is fixed into the female E, and is made a little hollow at its extremity F, so that it may be fixed to one of the screws V, T (Pl. I. fig. 2.)

391. Having carefully emptied such cylinder of air, and placed it in its proper place, I apply my hand to it and rub it; which is scarcely done, than I have the agreeable sight of my hand being represented on the inward surface of the same, by a red violaceous light, surrounded by another of a more vivid and whiter colour. The image of my hand thus represented, is delineated in Pl. IV. fig. 2; the fleshy prominent parts of it which touch the glass, are

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represented by a violaceous light, languid, rare, and uniformly spread with obscure points; the hollow intervals between the prominent parts that perform the friction, appear filled with a stronger and whiter light, which laterally extends itself along these same parts.

392. I have repeated the experiment after the same method used by Mr. Hauksbee, its inventor, with a cylinder inwardly lined with sealing-wax, with another lined with sulphur, and another lined with pitch; and in all these, when emptied of air, and rubbed in the same manner, I enjoyed the same sight as mentioned before; in all, the hand was represented on the thin inward lining: though, in order to see this, it was necessary to look through one part of the glass which had been left unlined: through the lining of sulphur, of wax, or of pitch, no light at all could be perceived.

393. I have just now explained how the fire deposited on the bands A, B, C, D, the act of friction, drives the natural fire from the inwards bands *a, b, c, d*; and how this fire successively runs through all the other subsequent bands; now, this same explanation will also serve to account for the wonders mentioned by Mr. Hauksbee with respect to the representation of an object within an opaque body, without the help of any looking-glass, or of any lamp or candle; and about the electric transparency of sealing-wax, of sulphur, and pitch; bodies through which no light from a candle can be perceived.

394. In fact, it is not light which traverses the substance of the cylinder, that produces the above representations, as Mr. Hauksbee thought it was; nor is it electric fire only vibrated in the place itself of friction (as I gave my opinion in the book of *Terr. Atmosf. Elect.*) it is electrical fire that is vibrated and driven from the inward bands *a, b, c, d*, where the outward ones A, B, C, D, first escape from the hand, and which runs through the subsequent bands of the glass, or of the linings (those linings being electric *per se*, and able to be charged like the glass, make one body with it) which runs, I say, after a most consequent manner, to form the above representation.

395. As soon as the rubbed glass leaves the hand, the electric fire deposited on it then accumulates, and affixes itself
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on it, and drives the natural fire from the inner surface: this fire thus driven, follows, on that inner surface of the glass, or of the linings, those parts to which the hand becomes by turns outwardly applied, and lightens exactly in the same manner as the electric fire does when flowing on the inner surface of a glass jar emptied of air; and if we apply to every rubbing part of the hand, what is here said of a few, we shall understand how the whole lucid image before mentioned arises, with all its peculiar circumstances.

396. Corresponding to that part of the finger which outwardly touches the glass and rubs it, the light will appear of a red violaceous colour, (we shall see afterwards how the rarity of the electric fire produces such colour), and besides be languid, rare, and uniformly spread with small obscure points; this is because both the small cavities in the fingers and the prominences on them, are in that place very close to each other; therefore there is scarcely time sufficient for the fire corresponding to such cavities to be vibrated and to lighten, as it presently finds itself carried over-against the rubbing prominences, correspondently to which it affixes itself on the inner surface.

397. But correspondently to those parts of the fingers which lie near their sides, and where the cavities we mention turn up round them, and grow wider, as there is both more room and time for the fire left by the prominences of the finger on the outward glass, to affix itself on it, and consequently for the inward fire to be driven from the inward surface, there this fire will flow more compact and copious on the said inward surface, and therefore will exhibit a whiter and steadier light.

398. But, after all, why does the fire vibrated from the bands, *a, b, c, d*, instead of spreading itself every where, continually follow, and keep to the subsequent bands *e, f, g, h*, &c. to which the hand becomes successively applied?

399. The reason is, that the fire, while it flows along these successive bands, meets with the least possible resistance. In fact, though the cylinder is ever so carefully emptied of air, there is always left within it some small remnant of it, which (supposing the common height of mercury in the barometer to be twenty-seven inches and an half, Italian measure,) will, if the vacuum be promoted

even so far as to lower the mercury to one-twelfth part of an inch, will, I say, be equal to one-three hundredth part of the atmospheric air : the resistance therefore which the vibrated fire will meet, when seeking to spread itself in every direction through this medium will be proportioned to its density ; and this resistance will encrease, according as the fire will seek to spread itself to a greater distance ; wherefore it must keep along the above-mentioned series of the bands *e, f, g, h, &c.* which continually afford it the nearest passage. With regard to flowing backwards along those bands which are just rubbed, the fire will be prevented from doing it by the same force by which it is driven from the glass ; that is, the same excessive fire that affixes itself to the bands A, B, C, D, and drives the natural fire from the bands *a, b, c, d*, will of course hinder it from returning to them ; therefore, the fire driven from the bands *a, b, c, d*, will meet with a greater resistance while it attempts to flow, either sidewise through the above-mentioned remnant of air, however rare it may be, or backward along the inward zone, than in immediately flowing from the precedent band *c*, to the following and contiguous one *d*, where no fire is yet affixed on the corresponding outward band D, since that band being, as we suppose, actually rubbed, has only a quantity of fire proportioned to its then encreased capacity.

400. The same reason holds true with respect to the lined tube before-mentioned, as well as with respect to the cylinder we are now describing. If the metallic linings of either this tube or cylinder only transmitted the electric fire along their outward and open surface, as is the case with glass, or insulating linings made of wax, sulphur, &c. no doubt but the same images would be perceived on those metallic linings as are to be seen on glass, or on the insulating linings we mention. And, vice versa, if either glass, or those linings, had the property of transmitting the electric fire to some distance forward, through their own inward substance, as metallic linings do, no image would then be seen upon them, any more than on metallic linings.

401. But let us proceed to describe the other accidents attending this experiment, which are not only both curious and useful to confirm the explanations already given, but also afford an explanation for

other phenomena, which yet seem at first to have no connexion with that same experiment.

Having sufficiently considered the lucid image or reflection of my hand, or other bodies apt to perform a friction, I open, for an instant, the little key D, (Pl. II. fig. 1.) and give entrance to a small quantity of air : which done, I observe, I. That the electroscope of the Chain already begins to diverge a little, though it did not move at all before ; and when I present my finger quite close to the Chain, I begin to see a small light manifest itself from it. II. The surface of the cylinder, where it leaves the rubbing hand, begins to attract a thread. III. The image of the fingers grows languid by degrees. IV. Instead of it, a light, *a, b, c*, shaped like a *nail*, (Pl. II. fig. 4.) begins to appear, and spreads itself over the inward bands, *a, b, c, d*, correspondently to the outer ones, A, B, C, D, as they part from the rubbing hand.

402. Now, the peculiar appearance of this *nail* of light, shews both its cause, and the cause of all the other concomitant alterations in the image within the cylinder. It is the result of the fire that is driven from the interior bands *a, b, c, d*, by that which now begins to accumulate itself on the outward ones A, B, C, D. The quantity of air that has been let into the cylinder does no longer allow this fire to pass so readily as it did before into the subsequent bands *e, f, g, h* : *a* part of it, indeed, is able to reach them, but then it only does it slowly in consequence of the encreased resistance it meets with ; and, as it is kept for a while from reaching the said bands, it forms, in the meanwhile, the appearance of this nail of light. As another part of the fire never can reach the said bands, but remains in the same place on the inward surface, and prevents the condensing of the fire on the outward one, the latter fire consequently begins now to manifest itself ; whence result both the divergence of the electroscope, and the other signs in the Chain, and more immediately the attraction of the thread by the cylinder. Lastly ; it is likewise evident, that since the interior flowing of the fire correspondently to the rubbing fingers begins to be hindered, the lucid representation of the latter must gradually disappear, since, even when the air is most dilated, this representation is but rare, and in some places spread with dark points.

403. When, for the fourth time, I let in another small quantity of air, the *nail* a, b, c, d , (Pl. II. fig. 4.) already spreads itself so far as an inch and an half, and is terminated in a sharp point; sparks may now be drawn off at the distance of more than one line from the Chain, and the outer surface of the cylinder begins to attract thicker threads. A new light, besides, is exhibited in the inside of the cylinder; flashes or rays of fire now spring from such inward points as correspond to those which outwardly get out of the friction, and crossing the rarefied air, run to the opposite inward surface of the cylinder, with a particular incurvated motion and divergence worth being described. Let a, b, c, d , (Pl. II. fig. 3.) indicate the section of the cylinder where it is rubbed; very near the point F, where the glass parts from the rubbing hand, (I suppose it moves according to the direction a, b, c, d ,) rays of light, f, g, h , are seen to spring and move with such an incurvated motion, as make it evident that the same is compounded of three motions, viz. of the rectilinear motion produced by the force that drives the fire; of the rotating motion of the cylinder, of which the ray, when issuing from it, somewhat partakes; and of the diverging motion by which the rays part from each other.

404. Of these motions, certainly the first and rectilinear one originates from the condensed outward fire, which drives those rays of the inward fire; in fact, those rays keep more close to each other according as the motion of the cylinder, and consequently the force arising from the friction encreases. The subsequent curvature of the rays is evidently the effect of the rotation of the cylinder, since it follows its direction, and visibly keeps pace with the rapidity of it: whence we may conclude that the velocity of this rotation is not in any very small *ratio* to the velocity of the rays themselves through the medium in which they move; so that, the former being *given*, we might conclude and find out the latter. Not that I think, however, that such rays are continuous, as they at first appear to be: the velocity with which children move lighted sticks, and make thereby their ends appear like continued igneous ribbons, is not, we may safely affirm, so great as that with which the rays of the electric fire move; therefore, a single flash or *darting* of this fire, though it may not of itself occupy a greater space than

than a twelfth part of an inch, may very well produce such a continued impression on the eye, as to appear a continued ray throughout the long space it passes through: which observation I thought necessary in order to hint what a small quantity of fire may be sufficient to raise the appearance of these long copious flashes of light.

405. It now remains that we should say something of the diverging motion of the rays which we have mentioned above; this motion, besides what it has in common with the motions of the electric fire through the atmospheric air, or through air imperfectly dilated, has moreover, on this occasion, a peculiar cause, viz. the electric atmosphere of the rubbed zone, which, when the rays begin to flash, begins also to acquire a degree of intensity; therefore this atmosphere must repel these flashes or rays: (in the following chapters I propose to shew how the direction of electric brushes, and also of sparks, is influenced by the repulsion of the electric atmosphere of the electrified body.) so that they diverge from each other very fast, and tend to move towards the lateral zones of the cylinder, which are not rubbed.

406. To a similar reason I impute certain flashes of light which I sometimes perceive in the inside of cylinders even when filled with atmospheric air. Sometimes, after rubbing those cylinders strongly, and for a long time, I perceive on that part of them which is distant about two inches from the place of friction, especially when I encrease a little the force of the latter, I perceive, I say, narrow flashes of light, which, from the inner surface of the glass, as far as I am able to perceive, run through a right line from the rubbed zone to the lateral zones that are not so, and there vanish. I think that such flashes of light are produced by the fire vibrated by that which outwardly accumulates itself in very great plenty; and also that this fire inwardly vibrated has a propensity to flow to the lateral parts of the cylinder, where no other fire is vibrated.

407. This conjecture I think I have ascertained; I perforated in *n* (Pl. III. fig. 7.) the wooden armature of a cylinder; I rubbed it for a very long time; I then took it from the screws; I inserted through the hole *n* the rod *nu*; I insulated and coated it, as I mentioned before of the cylinder that was inwardly lined with

metal ; and having properly adjusted the conducting bow C T C, I obtained the brush in C, and the little star in *c* ; so that even in such cylinder a degree of charge took place ; there was some excess on the outward rubbed zone, and a deficiency in the opposite surface : a certain quantity therefore of fire must needs have been diffused into those parts of the inward surface which lay near those from which the fire was driven ; and a certain degree of dampness in the air would have carried still farther this diffusion of the electric fire.

408. But even this last circumstance is not necessary : it is a very common thing in the driest weather to find remnants of electricity in cylinders, twelve, eighteen, or more hours, after the friction has ceased ; which never is the case with plates of glass rubbed on both their surfaces at once. It seems that such remnants of fire in cylinders proceeds from a kind of charge that takes place in them ; that is, from the natural fire which, being driven from the inward zone opposite to that which undergoes the friction, diffuses itself either into the lateral zones, or into those inward parts of the cylinder which correspond to the wooden armature ; and this fire, in the close air in the cylinder, returns but slowly to its former place. If, four hours after ceasing the friction, you give the cylinder half a turn, without touching it, you will find the electroscope of the Chain to diverge ; which will be owing to the cylinder having diffused into the chain an excess of fire adequate to such dose of natural fire as has slowly returned to its *natural* position in its inside.

409. But let us return to the experiment of Mr. Hauksbee. The nail of light *abc*, (Pl. III. fig. 4.) which, when a small quantity of air has been let into the cylinder, appears beyond the rubbing finger, affords a complete explanation of the light that appears in the barometer, and accompanies the mercury in it when it falls. I take the barometer and move it suddenly downwards, so as to make the mercury rise in it at once : as the same returns to its level, it is followed by a ring of light exactly like that which the *nail* forms within the cylinder, and both those lights vary in one and the same manner, according as circumstances change : that is, if in both the vacuum be complete, the light will be extremely weak ; and as a small quantity of air is let in, the same will lengthen and stretch it-
self

self under the shape of a nail; all other circumstances between the cylinder and the barometer will also be found exactly alike, and the same explanation will consequently serve for them all; the lengthened tract of light observed in the barometer is owing, as we said before of the *nail* in the cylinder, to that portion of fire which, through the new resistance from the air, can no longer readily follow the rubbing body, viz. the mercury in the barometer.

410. With regard to the attracting of surrounding bodies, the cylinder, we are to observe, begins to operate in proportion as the natural fire inwardly vibrated begins to follow less readily the rubbing body; because the outward fire, being thereby kept from condensing itself, manifests itself visibly, and attracts such light bodies as are placed near it. The same effects exactly take place, and for the same reason, in the barometer: as soon as, through the increased resistance of the air, the excessive fire within it can no longer freely and readily follow the mercury in its fall, it therefore, remains behind in a state of accumulation, and then begins to drive the natural fire from the outer surface; which fire in its turn manifests itself, and attracts threads and other light bodies.

411. Now to conclude our principal experiment. I let air into the cylinder for the fifth, sixth, and several other successive times, and the flashes of light gradually become more difficult to be produced; they now can only be obtained by stronger strokes of friction; they are shorter, narrower, though more lucid, and move with much more irregularity; which circumstances we shall find hereafter to be conformable to what I propose to shew in the IV. section with regard to the reciprocal action of air and electric fire upon each other. Here I only shall observe, that in proportion as more air is admitted into the cylinder, the electric signs grow more vivid in the Chain; but this only obtains within certain limits, for it appears that the signs attain their highest degree before all the air is let in; and, when it is, the same begins to weaken.

412. Now, this observation puts me in mind of the experiment of Mons. Du Faye, who could not, by friction, draw any electricity from a tube of glass within which he had condensed

air; from that experiment I was inclined to think, that the properest degree of density of the air within rubbed tubes for exciting a strong electricity, was under that of the common density of the atmospherical air; but the following experiment, which I have several times repeated, and always with the same success, has made me alter my opinion.

413. I made use of the long incurvated tube A, B, C, D, E, F, open in E, and closed in F (Pl. VI. fig. 1.) which serves me to shew, that our air, when unmixed and pure, *contracts itself into a space inversely proportioned to the compressing weights*; I loosened the shorter branch of it, F D, from the board to which it was fastened, and moved it from it, that I might more easily rub it; and the longer branch remained fastened to the same board. I could not, without danger of breaking it, rub this branch F C with any great degree of strength; however, by rubbing it with the amalgamated cloth, I could electrify it pretty well; I enabled it to draw threads at a foot distance, and it could give a few sparks. Having ascertained as well as I could the quantity of this electricity, I poured mercury into the longest branch CA, in a sufficient quantity to make it rise in the other branch CF ninety lines long, to *e*, that is to say, to the height of forty-five lines; by that means, the air in the tube was brought to a density double to that in the atmosphere. I then rubbed the part F *e*, and found that the electric signs from it, instead of being weaker, were stronger then before: I again introduced more mercury, so that there only remained in F *e* thirty lines that were empty, when I again found that the signs were still more vivid; and yet, the air was brought within the tube to a density treble to that in the atmosphere.

414. I suspect that Mons. Du Faye, and the other persons who have repeated this experiment, condensed the air in the tube with a syringe, the plug of which had been soaked with water; at least, when I have condensed the air with a syringe prepared with oil, I have been able to charge fully, as I mentioned above the glass-jar inclosed in the tube (Pl. VI. fig. 7.)

SECTION III.

On pressing electricity, or on electric atmospheres.

CHAPTER I.

On the first and principal property of electric atmospheres; and, on the other and secondary properties of the same, which immediately proceed from the former.

415. I. *THE electricity of a body A does not substantially diffuse itself into the ambient air; that is to say, if a body A be electrified by excess, the fire added to it does not mix itself, at least to any perceivable height, into the substance of the air around it; and if the body A be electrified by deficiency, the fire drawn from it has not been extracted from the substance of the ambient air. I think I was the first who demonstrated this fact (Art. Elect. p. 54.) I observed, that when I drew a spark from a conductor of gilt paste-board, small sparks were seen to shine in all these places where the gilding was interrupted. These sparks proceeded, I conjectured, from the excessive fire which, in electrifying this conductor, had by little and little diffused itself into such naked, resisting places; and which, running back afterwards into the neighbouring gilding, at the instant when the spark was drawn, sparkled proportionally to the resistance it met; therefore, I added, if the excessive fire that is introduced into a conductor, actually penetrates into the substance of the ambient air to any perceivable distance, it must in flowing back to that conductor through that resisting, though rare substance, likewise sparkle. This consequence was a necessary one; we see, lightning emits light whenever, in consequence of the scantiness of the substance that affords it a passage, it gushes into the ambient air. A strong spark while it passes through a stripe of gold, silver, &c. likewise exhibits a light, where the capacity of the metal is too scanty. It is a very agreeable sight to consider the effects produced by the discharge of a*

bottle through a chain of iron wire, hanging under a glass receiver, within which the air has been dilated to an half part of its density; this chain appears like a column every where surrounded by extensive winding flashes of light; which, besides melting and dissipating particles of the metal where the contact between the rings of the chain takes place, gushes more amply out of the same, in proportion as the air around it opposes less resistance.

416. If the body A were electrified by deficiency, the same reasoning to hold true needs only to be inverted; the electric fire in its returning into the substance of the air, whence it has been extracted, likewise emits light.

417. But the proofs of my proposition are, besides, completed by the following one, the truth of which has been demonstrated by Mr. Canton and Dr. Franklin (Franklin, p. 155.) viz. *The electricity of a body A, actuates the ambient air in such a manner, that by the means of the same it tends to introduce into the neighbouring body B immersed in it, an electricity contrary to its own.* I present a most fine flaxen thread to the conductor Y (Pl. I. fig. 1.) and if it be held at the distance of a foot, or more, from the surface of it, it will direct itself perpendicularly to it. I then present to this thread a rubbed stick of sealing-wax, and it flies from the conductor; I present to it a rubbed stick of glass, it runs again to it; which is the same as to say, the thread immersed in the atmosphere of the body or conductor A, which is electrified by excess, becomes itself electrified by deficiency. I repeat the experiment by the means, not of the Chain, but of the beams B and E of the Machine; then the thread flies from the glass, and runs to the sealing-wax; whence it is likewise evident, that a body immersed in the atmosphere of another body electrified by deficiency, will itself become electrified by excess.

418. III. *From the electricity of a body A, an electric atmosphere arises in the ambient air, in the same manner as, from the electricity which takes place on the one surface of a glass, a charge tends to take place on both its surfaces.* M. Wilke*, and M. Epino†, were the first who made

* See his Disputatio Physica de Electricitatibus contrariis, Rostock, 1757, p. 77.

† See his Tentamen Theorice Electricitatis & Magnetismi, p. 97.

the experiment of inclosing a stratum of air between two plates of metal, and they obtained a stroke from it; they observed, besides, that those two plates strongly attracted each other, as is the case with bodies *contrarily electrified*; that sometimes a spontaneous discharge took place, in the same manner as a vehement spark sometimes breaks even through the compact substance of a charged plate of glass; that a finger inserted between the two plates effected the discharge and was struck; that the discharge always took place from some prominence on the plates; that a point annexed to either of them hindered the charge.

419. But these facts do not afford a complete demonstration of the above proposition, and a farther confirmation of its truth may be deemed necessary. If the charge of a stratum of air be really a charge, it can be but a very small one, and it can scarcely exceed an ordinary spark; I have, therefore, sought for a method of examining whether such charge has the real characteristics of a charge, (viz. the accumulation of contrary electricities on the two opposite surfaces of the said stratum) and whether such electricities can only be produced and destroyed jointly. With three silk strings (Pl. VI. fig. 10.) I hung two round plates of tin, parallel, and distant from each other an inch or something more; and lest the electricity should dissipate from their sharp edges, I covered the latter with sealing-wax, and suspended both plates to the conductor with a small iron chain.

420. Then exciting the electricity in the conductor, and the apparatus of the two plates being in the meanwhile insulated, I observed that two extremely thin threads annexed to *cd*, amply diverged from each other, as did also two similar threads annexed to *AB*; and that the former, as well as the latter, were electrified by excess; as is the case with bodies immersed in atmospheres impregnated with excessive fire. I then insulated myself (Pl. I. fig. 1.) in such a situation as to have my face placed near the round knob by which the great conductor *Y* is terminated, and I stretched my arms parallel to the axis of the same; in such situation my legs and back became electrified by excess.

421. Secondly, I drew off all the electricity from the conductor, and when the threads were quite down, there remained in
me

me no sign of electricity. Likewise, when I grasped the conductor, being thus insulated, and having my face and arms near it, all electricity disappeared on my back and legs.

422. These two facts are the same as those observed in a bottle suspended to the conductor, and left untouched. The fire from the conductor endeavouring to accumulate itself on the inner surface of the bottle, drives an equal quantity of fire from the outer surface into the coating, and through it into the ambient air; and when the excess which endeavoured to condensate itself on the inner surface is stopped, the natural fire that had been driven from the outer surface into the contiguous air, falls back into the same. Thus the excess from the conductor endeavours, by the means of the upper plate AB, to condensate itself on the surface *ab* of the inclosed stratum of air, and vibrates an equal quantity from the inferior surface of the same, into the inferior plate CD *cd*, and through it into the contiguous air *cd*: the excess from the conductor being stopped, the natural fire falls back from the air contiguous to *cd*, into *cd*, and, through the plate, returns into the inferior surface of the air, which spreads over CD. The same must be said of the electricity that rises in my back and legs, and which, I being insulated, vanishes at the same instant that the electricity in the conductor is suppressed.

423. And as those electricities thus raised on bodies immersed and insulated within electric atmospheres, vanish as soon as the electricity of the body that actuates the latter vanishes, they may be called *electricities of simple pression*.

424. Let us now examine whether electric atmospheres produce such electricities as may really be called *charges*. I touch the plate *cd*, and obtain one spark from it, and no more; whence I conclude, that if the inclosed stratum of air takes a charge, it is but a very small one. In the same manner, when I stand insulated, as in No. 420, another person can hardly draw from me more than one spark; the same conclusion as that above is therefore to be drawn with regard to the stratum of air intervening between the conductor and my body.

425. Fourthly, I try to discharge the stratum of air by *alternation*. When I touch AB, I obtain a strong spark, but less than that

that which I drew when I touched the two plates at once, I then touch *cd*, the spark is still less; touching AB again, I draw another small spark, which I hardly feel with the tip of my nose; I touch AB a third time, and draw no spark, only the threads in *cd* manifest some inclination to move from each other; I touch *cd* a third time, and I have no spark, only the threads in *cd* move as if intending to join. Continuing to touch I can perceive no farther signs.

426. *I therefore conclude, that in the stratum of air inclosed between the two plates, there is a real, though indeed exceeding small charge; I say a real charge, because the contrary electricities of such stratum cannot be separately destroyed but by the means of an alternation; but this charge is, at the same time, very small, because the alternation does not extend, at least sensibly, beyond the third contact; and even so early as at the second, the spark drawn from AB is very weak.*

427. Therefore, we shall call *electricity from an actual charge* (though a very small one), that which takes place in a body immersed in an electric atmosphere, when such body communicates with another body.

428. The fluidity of air, which makes it liable to be traversed to a certain distance by sparks, prevents the charge of any thin stratum of that rare moveable element; and there is, on the other hand, just ground to suspect, that the degree of thickness which is necessary for preventing a stratum of air from being traversed by sparks, obstructs its charge, in the same manner as we see that insulating bodies are but difficultly and imperfectly charged, when they are thick beyond a certain degree. I intend to procure a large drum of crystal, an inch and an half high; I will fit to it as a bottom, a large plate of highly polished metal (surfaces thus polished do not throw sparks but from very short distances); as a cover or lid to that drum, I will fix on it a similar thick plate, but perforated in its center, so as to receive within its hole a rod of metal, so adjusted as to move in it without giving an entrance to the air; this rod is to raise and lower at pleasure, within the drum; another plate polished in the same manner as the bottom
of

of it. I moreover propose to condensate the inclosed air, and to examine whether greater charges may be formed by the means of a stratum of air thus condensated: by these means, the body of air to be charged may undoubtedly be brought to have a much less degree of *thickness* than in the before mentioned experiments.

429. Fifthly, and lastly, I demonstrate by another method the reality of the charge (though it be ever so small) of a stratum of air of a sufficient thickness. I take the plate $CDcd$ from the apparatus, and fix to it a stick of sealing-wax, by which I hold it; and then present it again, thus insulated, under the other plate $ABab$, which remains suspended to the Chain; and I soon see the threads in cd diverge and fly from a rubbed stick of glass; in a word, they are electrified by excess. I remove the plate $CDcd$, the threads instantly fall, and all electricity in the plate vanishes, which is the characteristic of an electricity of *simple pression* (423). I again present at a convenient distance the plate $CDcd$, under the other $ABab$, and touching the same in cd , I instantly complete the small charge, and the threads in cd fall down and remain in h ; but I no sooner remove the plate, than the threads begin to diverge, and fly from a rubbed stick of sealing-wax, and appear electric by deficiency. The cause of all this is, that the electricity by deficiency introduced into the surface of the air contiguous to CD , remained concealed, as long as it was in a manner retained back by the excess in the other surface of the said stratum contiguous to ab ; but as soon as the plate CD is removed, this deficiency suddenly manifests itself.

430. This, at the same time that it demonstrates to me the reality of the charge of a somewhat thick stratum of air, confirms also to me the conjecture I have proposed elsewhere, with regard to the charges of insulating bodies in general, viz. that the quantity and connection of the two electricities that constitute a charge, depend on their contrariety to each other, and reciprocal combination on the opposite nearest surfaces of the charged body, and do not in any shape arise from any peculiar (whether positive or negative) property in that body. In fact, if such a small electricity *from a charge* as that in the plate $CDcd$, becomes so sensibly manifested when that

plate

plate is removed from the plate $ABab$, with how much greater a degree of force would not the infinitely superior contrary electricities of a plate of glass, become manifested, if they could be separated from each other in the same manner? Certainly the connection between the two contrary electricities that constitute a charge, depend on the above mentioned combination: so long as the two tin plates remain near one another, and the two electricities on the two surfaces of the closed stratum of air remain thereby combined together, these electricities cannot be destroyed otherwise than by an *alternation*; but as soon as, by removing the plate $CDcd$, this combination is destroyed, either of the two electricities may be suppressed by a single contact.

431. This idea of considering the stratum of air $abCD$ (Pl. VI. fig. 10.) as being really charged, naturally leads us to apply the principle of an electricity by *simple pression*, to any *surface of air* contiguous to electrified bodies. Thus, for instance, when the electricity of the conductor to which the two plates are suspended, begins to penetrate the plate $ABab$, the other plate $CDcd$ being untouched, it would, in this case, be more accurate to say, that the excess that gets into the plate $ABab$, tends, it is true, to condensate itself on the surface of air contiguous to ab , endeavouring, at the same time, to drive the natural fire from the surface of air contiguous to CD ; but that it only can drive the natural fire from the surface CD of the plate $CDcd$, which, through the same, condensates itself on the air contiguous to cd , because the excess maintained in cd does not allow the natural fire to part from the surface of air CD ; and, as a necessary consequence, prevents a condensation of the excessive fire on the surface ab , &c. According to such accurate manner of expressing the case we indeed are to understand what has been related in No. 422, but every body sees what a confusion would ensue from a strict observance of it; therefore, I beg leave usually to express myself more concisely, and only to consider the electricity that takes place in the bodies themselves that confine electrified strata of air. Conformably to such licence, I shall simply say, that the electric fire which is accumulated in a body, endeavours to drive the natural fire from another body that is not electrified; and

B b that

that the natural fire rendered deficient in a body, endeavours to make a correspondent quantity of excessive fire accumulate on the surface of another neighbouring body that is not electrified.

432. And availing myself of the above shortness of expression, I shall proceed farther, now that I have examined into the first property of electric atmospheres, viz. *that the electricity of a body endeavours, through the atmosphere which it actuates, to introduce a contrary electricity into a body immersed in it*: I shall proceed, I say, and try to describe the two secondary properties, which necessarily proceed from the above mentioned.

433. Of these the first is, that, *when two bodies, impregnated with homologous electricities, meet, these electricities, by means of the atmosphere which they actuate, endeavour reciprocally to destroy each other*. The reason is, because the electricity of each of these two bodies, endeavouring to introduce a contrary one into the other body, will, consequently, begin with destroying the homologous electricity it finds in it; therefore, if such electricities, besides being homologous, are moreover equal, they will at first lessen, and then, if they are at such distance that they can sufficiently exert their mutual influence, intirely destroy each other.

434. If the two electricities are in unequal quantities, the remnant of the more copious electricity will be employed in producing a contrary electricity in that body, the electricity of which has been destroyed.

435. The other secondary property of electric atmospheres is that, *when two bodies impregnated with contrary electricities, meet, these electricities, by means of their intervening atmospheres, reciprocally increase their intensities*. This is plain; the same force which endeavours to produce a contrary electricity, where none exists yet, must likewise endeavour to increase the same when it already obtains.

436. But let us return to the principal property, and express it with more exactness, applying it to the two electricities by excess, and by deficiency; and let us examine whether from such exactness we may derive some less abstracted idea, and that may be represented by some sensible image. The aforesaid principal property is, therefore, to be expressed thus, with respect to an elec-



Fig. 3

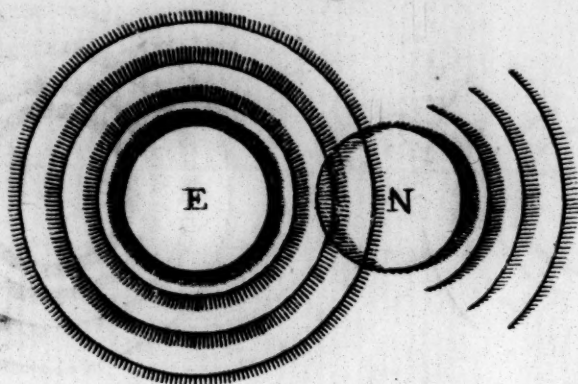


Fig. 5

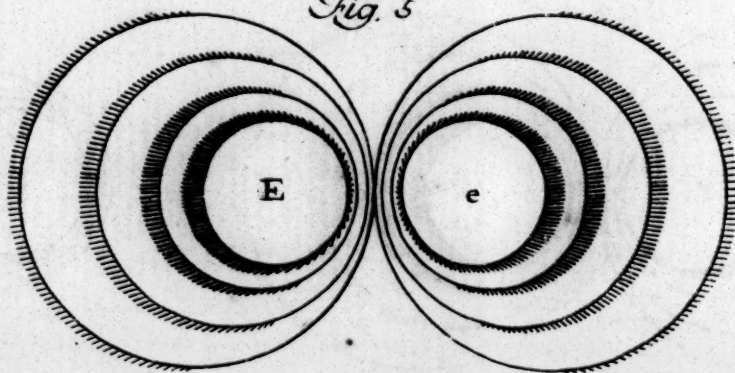


Fig. 9

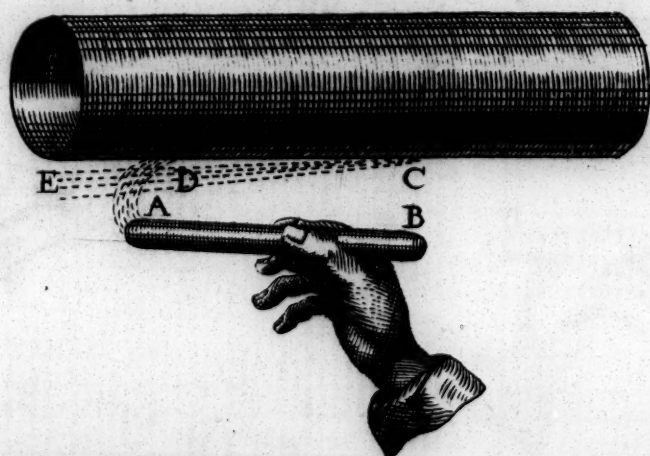


Fig. 1

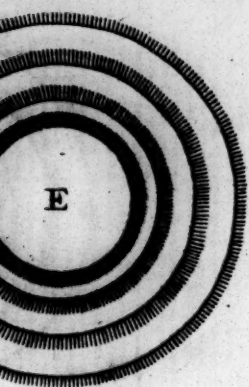


Fig. 2

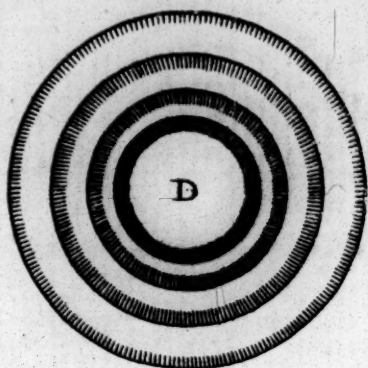


Fig. 4

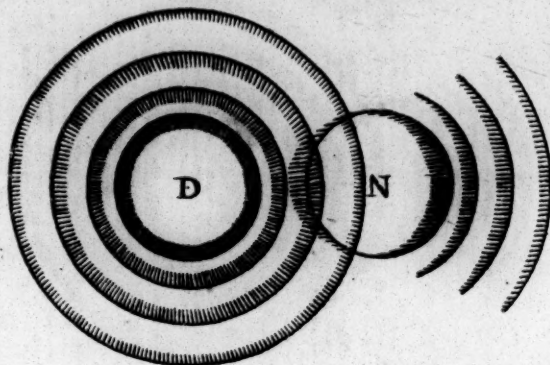


Fig. 7

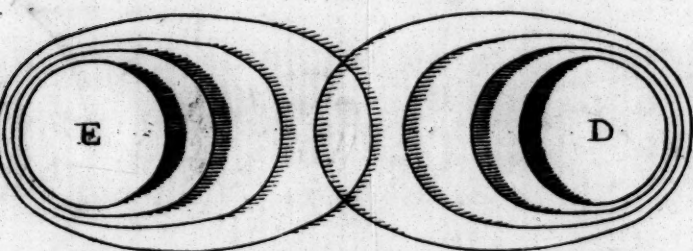


Fig. 6

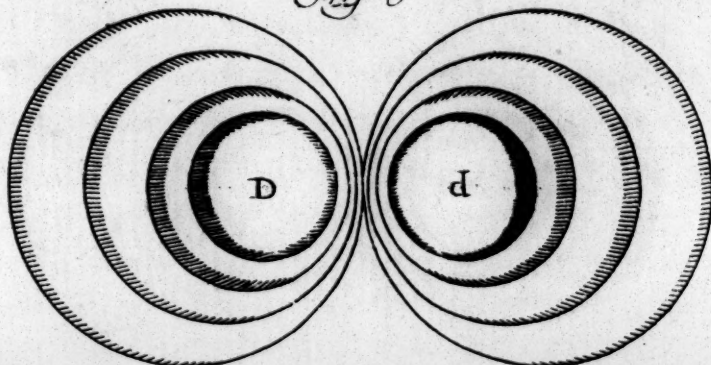
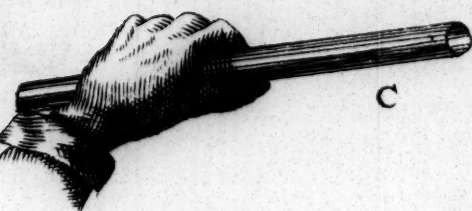
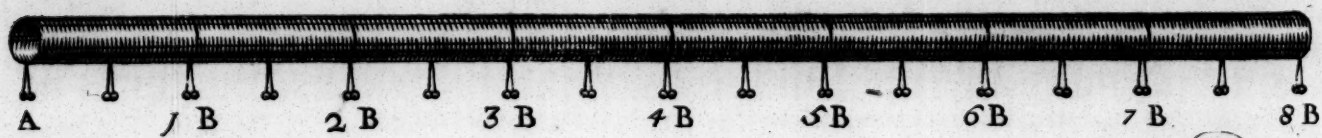


Fig. 8





electricity by excess; *if a body possesses any portion of excessive fire, the latter will actuate the ambient air in such a manner, that it will endeavour to drive the natural fire from such bodies as are immersed in it.* Now the existence of an excess of electric fire implies a greater density of it, a greater expansive force in it, a greater tension of the same, whatever it may be; and must not such tension introduce a greater tension also into the ambient air, and cannot we consider such tension thus ordinally propagated through the fire inherent in the successive parts of the ambient air, as serving to vibrate the natural fire from the air immersed in it? Therefore, if a body E, be electrified by excess (Pl. VIII. fig. 1.) first a set of small lines springing against the ambient air from all points of the surface of the said body E (we shall see hereafter that both the excess and deficiency of electrified bodies only take place on their surface) may very well represent both the excessive fire of such body, and the increased tension exerted by the same fire against the natural fire of the ambient air; secondly, the ambient air may likewise be understood as being divided into successive exceedingly thin strata, and similar small lines may also, being understood to be applied to the convex surface of all those strata, express the direction and manner in which the excessive tension of the fire on the surface of that body, propagates, &c.

437. If we apply to cases of deficient or negative electricity, the above principles concerning the first property of electric atmospheres, we ought to say, *if a quantity of natural fire be taken from a body, the air around it will from thence be so actuated, that a quantity of excessive fire will run to another body immersed in that air, &c.*

The Italian Author, in the two foregoing paragraphs, and in the three following ones, attempts to describe mechanically the action of electrified bodies upon the air around them, and the action which that air, or atmosphere, in its turn exercises on such bodies as are immersed in it. In these mechanical descriptions, or explanations, the Author is greatly assisted by the diminutive expressions with which his own language supplies him; but the want of such expressions in English, together with the great difficulty that always attends abstracted descriptions, or rather conjectural reasonings, of this kind, has induced us to pass over

the said paragraphs, and to go to the 441, by which this chapter is terminated. In this paragraph our Author says, that in case it should be found that in the mechanical description he has just attempted to give of the manner in which electric atmospheres both are actuated, and act, he has too much indulged his own imagination, as well as applied to that of his readers, he now is going to make amends for it, by elucidating the properties of those atmospheres from a number of accurate experiments.

C H A P. II.

In which the three above properties of electric atmospheres are farther demonstrated, by experiments chiefly made within cavities in deferent bodies. The superficiality of the electricity of deferent electrified bodies is demonstrated. And a few conjectures are offered to the reader concerning the proportional quantity of the electric fire within the substance of deferent, and of insulating, Bodies.

442. **I** Call *electric well* (Pl. VII. fig. 1.) the vessel, or cann A, made of tin, fifteen Italian inches high, and six and an half wide; and, to prevent its losing its electricity, its edge has been rounded by the means of an iron ring fixed to it. I insulate this cann, or *electric well*, upon a small table T, raised upon a support of glass V, and I commonly electrify it by touching it with the hook (or with the coated bottom) of a charged bottle, which I hold by the means of an insulated handle (Pl. III. fig. 3.): I need not say, that when I touch the *well* with the hook of the charged bottle, I observe to touch with my finger the outside of the same, and vice versa, to touch the hook with my finger, when I mean to charge the well with the outside of the bottle. In this well I distinguish two parts; the first is the *lower* part of it, that is, that part of its cavity which reaches from the bottom, to two third-parts of the total altitude of it; the *upper* part is that which, from thence, extends up to the edge of the well.

443. I call *scrutator* an electroscope annexed to a long stick of sealing-wax (Pl. VII. fig. 1.), the threads of it are exceedingly fine,

fine, and only an inch and an half long, and to them are fastened two bits of paper in order to render their motions within the cavity of the well sufficiently conspicuous; and that I may perceive them the better, I cover the bottom of the well with a round plate of tin blackened over.

444. I. A man suspends the scrutator in the middle of the lower cavity of the well, in such a manner that it does not touch either the bottom or the sides; I then touch the well, at one time with the hook, and at another with the outside of the bottle, when I find that the threads remain unmoved. II. The person who holds in his hand the threads of the scrutator now touches the bottom, and then the sides of the lower cavity, and the threads still remain unmoved. III. The same person suspends again the scrutator in the middle of the lower cavity, without touching either the sides or bottom of the well; I then insert into the well a small rod of brass, with a ball at its end, and present it to the threads of the scrutator, taking care not to touch either the edge or sides of the well, and the threads fly to the ball. I destroy the electricity, and the person who holds the scrutator draws it out of the well; then the threads manifest an electricity contrary to that which I had communicated to the well; that is, if I touched the well with the hook, the scrutator runs to the hook, and flies from the outside of the bottle; if I touched with the outside of the bottle, the scrutator flies to the outside of the bottle, and runs from the hook. IV. I annex a short and very fine thread laterally to the lower cavity of the well; the scrutator is again suspended within it, and I, with the bottle (I always understand it to be strongly charged) electrify the well. Seeing that both the threads of the scrutator, and the annexed thread remain unmoved, I put the brass rod into the well, and present it to the threads of the scrutator, when the latter instantly run to the rod, and the annexed thread diverges a little from the well; if both the annexed thread and those of the scrutator happen to be near each other, they immediately join.

445. I shall relate another experiment on the same subject, the consequences of which are still more obvious than those of that just described. I fasten to three silk threads B, a cylinder of tin

(Pl.

(Pl. VII. fig. 2.) two inches high, and also two inches wide, the edges of its basis are rounded, that it may not attract electricity, and I call such cylinder the electric bucket. I. A man suspends this bucket within the lower cavity of the well, taking care that it does not touch either the sides or bottom of it; I electrify the well, the man draws the bucket out, taking care to keep it at an equal distance from the sides; I then present a thread to it, and it is not moved in the least. The man puts again the bucket into the lower cavity of the well, of which I revive the electricity; he draws it off again; I then present a thread to it, and it still keeps unmoved. III. The man again puts the bucket into the lower cavity of the well; I electrify the well, and then put into it the rod C, the ball of which I present to the bucket; then a spark is thrown out of it, which, with respect to its explosion and light, is much superior to what might be expected from the capacity of the bucket. The man again draws the bucket out, I present the scrutator to it, and the threads are rapidly drawn, then repelled by it. When I have touched the well with the hook of the bottle, the threads repelled by the bucket, are also repelled by the outside of the bottle, but drawn by the hook; when I touched the well with the outside of the bottle, the threads repelled by the bucket, are also repelled by the hook, but drawn by the outside of the bottle. IV. The man suspends within the well two buckets of an equal size D, *d* (Pl. VII. fig. 3.), the bucket *d* hanging from the bucket D, by silk threads two inches long: the bucket *d* is lowered till it actually touches the bottom of the well; both are drawn out, and neither of them can move the threads I present to them. V. The man again puts the buckets into the well; the lower *d* touches, as it did before, the bottom of it, which D does not; I then touch D with the rod, and a strong spark is thrown out; the man draws out the two buckets, when both draw the threads, but D does it most strongly, and *d* but very weakly. VI. The experiment is repeated, and the buckets are drawn out; I present to D the threads of the electroscope, after they have touched the part of the bottle contrary to that with which I electrified the well, and they are repelled; I then with my finger touch the bucket D, and these same threads which are strongly repelled by D,

D, are weakly drawn by *d*. VII. I again repeat the experiment : the buckets being drawn out, I suddenly destroy the electricity in D, and the threads of the scrutator, which have touched that coating of the bottle with which I have electrified the well, are repelled from *d*.

446. Dr. Franklin, in page 325 of his works, proposes his experiment of a cork ball, which, hanging by a silk thread, and lowered into a silver can till it touches the bottom of it, draws no electricity from it. I was informed of this experiment by a short but ingenious dissertation of M. de Saussure, a philosopher of Geneva, when, in February 1769, I published my little book *de Atmosphérâ Electricâ*, in which I fully analysed this surprising and mysterious experiment. I have since received the Work of Dr. Priestley, published in the year 1767, in which I have seen that the ingenious author had made several attempts (page 731.) to analyse the same experiment, though he had nowise succeeded ; this I attribute only to the extreme delicacy of the subject, which requires both a most favourable weather, and a most careful management throughout the whole experiment. The weather being supposed favourable, here are a few things amongst others, that must be attended to : I. The bottle with which the well is to be electrified, must, instead of a hook at the end of the rod inserted into it, have a pretty big ball fixed to it, because the hook which is commonly used, being made of a thin rod terminating in a point, may easily turn the electricity in the threads of the scrutator into a contrary one. II. When afterwards the electricity is to be excited in the threads of the scrutator, they must at the same time that they are presented to the outside of the bottle, or to the hook (I retain the same name though a ball is now used), be rapidly touched with a finger, that they may thereby, first, contract a contrary electricity, and then fly to those bodies and impregnate themselves with their electricity. III. Before drawing either the scrutator, or the buckets, out of the well, the electricity of the latter must be destroyed, lest those bodies in their passing through the mouth of it, should receive an alteration in their electrical state, from the united atmospheres of the edge. IV. But the chief reason why, when the two buckets are drawn at once, it becomes

becomes necessary to suppress the electricity of the well, is to prevent a mixture of the two different electricities; that is, the strong one in D, and the weak one in *d*: it is needless to observe, that before suppressing the electricity of the well, the bucket *d* must be previously raised somewhat above the bottom, else its electricity would be destroyed also. V. Moreover, when the two buckets have been jointly drawn out, it must be observed, that as long as D will retain the electricity it has contracted within the well, the bucket *d*, which will be surrounded by the atmosphere of D, will repel the threads of the scrutator when they have been repelled by D; therefore, the electricity of D must be previously destroyed to inquire after the contrary, and weaker one in *d*.

447. These cautions being once carefully attended to, the experiments that I have described are always followed by the same effects, and afford an analysis of the said fine Franklinian experiment, as well as complete the demonstration of the property of electric atmospheres, which I deduced in the precedent Chapter from the simple experiment of a thread presented either to the Chain, or to the Machine: the above experiments, besides, open a vast field for interesting and important discoveries.

448. All these experiments concur in making it manifest, *that the electricity introduced into the well, endeavours, it is true, to excite a contrary one in the threads of the scrutator, or in the bucket, or in any other body placed within its cavity* (in which consists the first property of electric atmospheres.) II. *But that the homologous electricities that seek to communicate themselves to the above bodies from opposite parts of the cavity, reciprocally obstruct and annihilate each other* (which is the second property), as long as those immersed bodies have no communication either with the ground, or other external body, by the means of which they may be enabled to contract an electricity contrary to that in the well.

449. Secondly, from the same experiments, a few additional truths are moreover discovered, concerning the above properties of electric atmospheres; for instance, they shew *that an excess of fire endeavours to produce a deficiency equal to it, and vice versa*. If this were not the case, how could it happen that the electricity of
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the bucket *d*, which touches the bottom of the well, comes out of the same with an electricity so inferior to that of the bucket *D*, which remains suspended within the cavity of it? surely this must be imputed to the aforesaid equality. From the inner surface of the well, no quantity of fire can be exerted greater than that which can be driven from the small surface of the bucket *D*; or, if the well be negatively electrified, from the inner surface of the well, no quantity of natural fire can pass to the outer surface of the same, greater than that of the excessive fire which can flow to the inconsiderable surface of the bucket *D*; therefore, as the whole capacity of the inner surface of the well, is to the capacity of the bucket *d*, so the intensity of the *excited* electricity (whether positive or negative, which all lies in *D*) is to the intensity of the electricity in the bucket *d*, into which only a proportional part of the *exciting* electricity is diffused.

450. Fourthly, from the same experiments we may again draw this consequence, viz. that *the electricity which arises in the bucket immersed within the cavity of the well, must be as much superior to the electricity that would take place on the same, if immersed in the outer atmosphere of the well, as the number of points in the cavity that concur to actuate a contrary electricity in the bucket, is greater than the number of points that endeavour to introduce a contrary electricity into the same, when suspended in the outer atmosphere.* In fact, the bucket outwardly suspended gives a spark much weaker than that which is thrown out by it, when it is placed within the cavity of the well, &c.

451. In the third place, it follows from the same experiments, that, *both the quantity of electricity that will be exerted from the cavity of the Well and the atmosphere that will be excited by it, as well as the contrary electricity that will rise in that part of a body which is only partly immersed in the Well, will all of them be in proportion to the capacity of that part of the same body which is not immersed.* With three silk strings joined together in *B* (Pl. VII. fig. 4.) I suspend within the well *A*, the cylinder *C*, the edge of which is rounded at its basis, and I keep it at an equal distance, that is two inches, both from the sides and the bottom. I electrify the well, and that part of the cylinder *C*, which is not immersed, repels

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the same electroscope that is repelled by the well; it possesses, therefore, the same kind of electricity. This may be easily explained: the electricity of the well, by raising an electricity contrary to its own in the immersed part of the cylinder, necessarily introduces a contrary one (consequently one homologous to its own) into that part which is not immersed; or, in other words, the natural fire driven from the former part of the cylinder goes into the latter; or, if the well be negatively electrified, the natural fire runs from the part which is not immersed and accumulates itself on that part which is immersed. Therefore, according as the capacity of that part of the cylinder C which remains out of the well in the open air (it must be observed, that the air near the mouth of the well is somewhat affected by the atmosphere, both of the edges, and of the upper part of the cavity, which obliquely exerts itself) will be greater or less, both the excited and the exciting electricities, and the atmosphere through which this excitation is effected, will also be proportionably stronger or weaker.

452. From the same experiments it results also, *that electric atmospheres exert themselves and operate obliquely*; this I have just now taken for granted, I will now try to demonstrate it. Let us for an instant suppose that electric atmospheres only operate directly; if so, the electricity from the bottom of the well will suffer no kind of counteraction, while the sides of it will counteract each other; an electricity will, therefore, be able to spring directly from the bottom of the well, and none will be thrown from the inner edges, since every point of them has some other point directly opposite to it; but now, this is not by any means the case; we must, therefore, acknowledge, that the electricity that is thrown from the bottom is counteracted by that which obliquely springs from the sides of the well, directing itself towards that same bottom; and also, that a certain quantity of electricity really exerts itself in an oblique direction, from the inward edge or mouth of the well. All this may be very easily ascertained by the following experiment. Let a number of short threads, about an inch and half long, be, both outwardly and inwardly, annexed to the sides of the well; let them be disposed in a vertical line, so as
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to form as it were a single continued thread. When the well is electrified, those threads which stand within the lower cavity of the well, keep their former situation; those which are adapted to the outside of it, are all raised into lines perpendicular to the sides of the well, though with a small deviation from each other, by which the one are inclining to the right, the other to the left side alternately: those which are annexed to the upper part of the inside of the well, diverge more or less from it, according as they stand nearer to the mouth; and if any threads have been annexed to the bottom of the well, they remain entirely unmoved.

453. This oblique action of electric atmospheres explains how it comes to pass, *that the atmosphere of the one surface of an insulating plate, extends itself so far as to reach the other surface of it*; and from thence comes to be completely understood, one of the most perplexing experiments that occur in the science of electricity. The experiment has been first made by Mr. Richman: M. Epino, as I understand from Mr. Priestley's Work, has related it, but as I have not been able as yet to procure his book, I shall describe the above experiment in the manner that it has constantly succeeded with me, and conformably to the analysis I have made of it. Let AB (Pl. VI. fig. 13.) be a glass plate nine inches long, and seven inches wide; let MN and OP represent its coatings; CD, EF, are two very thin threads, an inch and an half long, with stripes of paper hanging from them, and they are annexed to the middle points of both coatings; I then insulate the plate on a stick of sealing-wax, and make the coating MN communicate with the Chain, though in such a situation that the electricity from the Chain do not counteract the electricity which is to rise in the thread CD. From the ground I touch the other coating PO, taking care to keep my finger at such a distance O from the thread EF, that it may not influence its divergence; and then I observe, I. That in proportion as the charge is farther promoted, both threads CD, EF, gradually acquire a greater divergency. II. That if, in this state of things, a person insulated and communicating with the Chain, presents the palm of his hand parallel to the plate, near the thread CF, the latter falls; and if from the ground I likewise present my hand to the thread EF (still touching the coating in O), the thread E falls likewise. If both the hands that are presented to

the threads, or either of them, are removed, both the threads, or either of them, begin again to diverge. III. If, when the charge is completed, I present my hand to the thread EF, it falls down, though I entirely leave off touching the coating with my other hand; but, on the contrary, if before the charge is completed, I present my finger to the thread, and leave off touching the coating, it flies to my finger.

454. All the difficulty arising from this experiment is resolved by supposing (which is the truth), that the electric atmosphere, which (while the charge is advancing) is excited from the coating MN, bends its course, and actually extends itself to the air contiguous to the other surface of the plate; hence both the coating PO, and the thread EF, being immersed in air actuated by the electric atmosphere from the opposite surface which is positively electrified, become electric by deficiency; therefore, the thread EF diverges from the coating. But when afterwards I oppose my hand to this thread EF, my hand also becomes electric by deficiency; hence the homologous electricities of the coating PO, and of my hand, opposing the action of each other, the thread between them falls. When I present my hand to the thread EF, and cease at the same time to touch the coating, then the thread EF runs to my hand; the reason of this is, that the natural fire which is continually driving from the coating to which the thread is annexed, runs, the other communication being now removed, to my hand through that same thread. But when the charge comes afterwards to be completed, as no fire is any longer driven from PO, then, even though I cease to touch the coating PO, the thread EF, the coating itself, and my hand, all remain electric by deficiency, though the continued action of the electric atmosphere of the other surface MN, which, as has been observed above, continues to bend its course, and to flow to this surface PO.

455. I demonstrate the truth of the above explanation, by experimenting with the large plate ABCD (Pl. II. fig. 8.) I annex two stripes of gold leaf to the margin of its inferior coating, and place them parallel, and near to each other; I annex two other stripes to the same coating, and place them somewhat more advanced within the compass of it; and two others besides, at a still greater distance from the margin; this inferior coating is then

then made to communicate with the ground, and the upper coating with the Chain; when I observe, I. That in proportion as the charge advances, the electroscopes, or stripes, annexed to the inferior coating, gradually acquire some divergence; but with this difference between them, that those which stand nearest to the margin of the coating, manifest first their divergence; then those which follow them; and, last of all, the third stripes begin to manifest what small degree of divergence they may have acquired. II. The charge being completed, the greatest divergence still continues to take place in those stripes which stand nearest to the margin; a less divergence obtains in those stripes which have been placed in the middle; and the least divergence is manifested by the remotest stripes. III. All those stripes fly from my finger, and those which diverge least, increase their divergence when I place my little finger between them. IV. All fly from the outside of a bottle inwardly charged by excess. From these facts I therefore conclude, that all the stripes are electric by deficiency, as also is my finger (though no doubt in a less degree than the above mentioned bottle). The reason is, that both they and my finger are immersed in an atmosphere actuated from an excessive electricity, which, bending its course, flows to the inferior coating to which those stripes are annexed, and introduces a greater or less degree of electricity into them, according as they are situated nearer to, or farther from, the edge of the plate. That a body immersed in a given atmosphere acquires an electricity contrary to that by which this atmosphere is actuated, is what I think I have demonstrated before, and what every body may easily ascertain. Present a very fine thread to the Chain, and when it begins to move towards it, present your finger laterally to this thread, it will fly from your finger.

455. Fifthly, We may conclude from the above experiments, that *from hollow surfaces imperfectly closed, an electricity will be manifested, which will be less in proportion as those surfaces are nearer being completely closed; and in proportion as they change either to plane, or to convex, or to more convex, surfaces, an electricity will be manifested around them in greater plenty and augmented force; this is, because in proportion as the surface will be less hollow and closed, and will approach nearer to being plane, the reciprocal counteraction of the*
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atmosphere actuated from different points of this surface, will lessen. But as atmospheres, as we have just now observed, act also very obliquely, and even so much so as to bend their course from the one side of a plate to the other, always preserving their property of exciting an electricity contrary to that by which they were actuated in the beginning, it hence follows that electrical atmospheres from plane surfaces, counteract each other in a less degree than those which spring from concave surfaces, though in a greater degree than those which spring from convex surfaces; so that an electricity that rushes from an infinitely convex surface, that is, from a very sharp point, placed at a great distance from any other surface that may be animated with an electricity homologous to its own, will be counteracted the least possible, will be thrown out with the greatest force, and will exhibit either the brush or the little star. But of these important conclusions, I only mean in this place to drop a seed, or hint; let us for the present proceed to reap other fruits from our analysis of the electric well*.

456. Of such fruits or discoveries, the following is certainly a very valuable one, viz. *that we have at length ascertained that, in pressing electricity, any excess of fire, any deficiency of the same, any kind of electricity in short, is confined to the free open surface of bodies, without in the least diffusing itself into their substance.* I say in *pressing* electricity, because with respect to the *vivid* electricity of a spark, there is no doubt but the latter condensates itself for an instant within the pores of bodies, and endeavours to break the cohesion of their solid parts. With regard to common, *pressing*, electricity, if no electricity can by it be made to adhere to the wide cavity of an electric well, how can we imagine that any electricity will better be brought to adhere and be accumulated within the narrow cavities of the inward pores of bodies? We cannot, it is true, suppose that within the substance of bodies there is any such medium as air, by means of which opposite atmospheres may

* If the reader entertains still any doubt about the reality of the *oblique* notion of electric atmospheres, the following experiment will remove them. Let him place one of his hands open under the great conductor, with a thread hanging from the middle of it; he will find that this thread becomes electrified by deficiency, through the *oblique* action of the atmosphere of the conductor, as well as a finger of his other hand, exposed to the direct action of this atmosphere; and, in fact, if he presents that finger to the thread, the latter will fly from it.

be actuated ; but yet, do not the solid particles that constitute the partitions between the pores of such compact bodies, for instance gold, stand much closer to each other than do the solid particles of air ? Must not, therefore, the same counteraction and opposition take place between the portions of electric fire diffused within the pores of a piece of gold, as obtains, by means of the fire inherent in the substance of the air, between the homologous electricities that endeavour to accumulate themselves within the cavity of the well ?

457. On this occasion, I shall remind the reader of the observations formerly related in page 76 of *Artificial Electricity*. A cube made only of gilt paper both attracts and gives sparks with the same degree of force as a similar cube of solid iron. Here too comes in its proper place the experiment of Dr. Franklin, who saw an electricity exerted on the surface of a metallic Chain, grow more vivid in proportion as a greater portion of the said Chain was gradually made to gather within a tumbler. I have imitated the same experiment after another manner. I adapt fine and short threads to the body of a Man, to his forehead, to his back, to his legs, to his arms, &c. I bid him stand, with his arms and legs stretched as much as he can, on two insulating stools, and then give him a spark from a bottle strongly charged ; when I find that the threads immediately diverge ; though most of all, those which are least counteracted by the electricity of other adjacent parts. The Man afterwards joins together both his legs on one stool, drops his arms, and at last gathers and folds his body in the same manner as tailors use to do ; then the threads placed in those parts which are become close to each other, fall down ; and those, on the contrary, which remain exposed to the free open air, increase their divergence. I repeat the experiment in a contrary manner, and give a spark to the Man when he is thus gathered ; and as soon as he begins to unfold and stretch himself, the divergence of the threads in the open air lessens, and those situated between joined parts of his body, acquire a divergence, according as they gradually become separated from each other.

458. If to these observations we add, that any spark, even the smallest, given to, or drawn from, a large conductor, always is seen to cause a sensible excess or deficiency in the threads, though
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ever so thin and comparatively small, that may be annexed to it; that any electricity excited in one part of an extensive conductor by any kind of electric atmosphere, becomes manifested on the whole part of that conductor which is immersed in that atmosphere, and excites an equal, but contrary one, on that part of the same which is not immersed, then, we shall be convinced, that Electricity does not diffuse itself into the substance of Bodies; or, in other words, that the electrical fire is not condensed or dilated within the inward substance of electrified Bodies.

459. But, lastly, if electricity did really penetrate into the inward substance of electrified bodies, surely when I lower the bucket into the electrified well, and make it touch the bottom, a part of the electricity which, in the case we suppose, is diffused within the cavity of it, would also diffuse itself into the bucket, and be manifested on it when I extract it from the well; now, when the operation is carefully made, the bucket constantly comes out of the well without the least electricity.

460. Lastly, The same experiments explain the following paradox, viz. *that the natural quantity of fire contained within insulating bodies, is incredibly greater than the quantity contained in deferent bodies.* This principle becomes farther confirmed, when we consider, that an hundred insulated Men can scarcely supply the surface of a bottle with the necessary fire to charge it; neither could they receive the whole quantity that escapes from the outside of the same bottle. The same thing obtains also with regard to the discharging of the bottle; an insulated Man touches an hundred different times the coating of a charged bottle, the other coating of which communicates with the ground; and, though after every touch he takes care to dissipate the fire he has received (or to recover that which he has given), he never has done exciting fresh sparks from the bottle; whence I was induced to conclude that the bottle could give or receive more fire than an hundred Men could receive from, or give to, it; and, consequently, that the natural quantity of fire in an ordinary glass-bottle exceeded that contained in an hundred Men.

461. But though we have demonstrated this truth, that the natural quantity of electric fire remains *unaltered* within the substance of *deferent* bodies, yet we have nowise demonstrated this, viz.

viz. that glass actually contains more fire than deferent bodies do. A man, for aught we know, may contain a very great quantity of electric fire; this only is ascertained, that he can give to the bottle no other fire than what he can himself receive from other bodies, or receive from the same no more fire than he can transmit to other bodies. With respect to the air around him, as the *charge* of that element can be but very small, a man needs only receive a very small quantity of electricity from it, to exhaust it; therefore, in this case likewise, he may actually contain a very great quantity of fire, though he only gives to the air around him, or draws from the same, an exceeding small quantity. In order to be able to draw a great quantity of fire from the air, or give it to the same, a man must communicate with an extensive system, from which he may draw, or to which he may transmit, such fire.

462. The above is the essential difference between *insulating* and *deferent* bodies. With respect to the former, such is the consequence of their electrical *impenetrability*, that great alterations in their electric fire may be introduced in their opposite surfaces; and such alterations are both equal to each other on the respective surfaces on which they take place, and contrary; but in *deferent* bodies, no such alteration can be effected, on account of their *penetrability*; though, for aught we know, the density of the fire in deferent bodies may very well be equal to the natural density of it in insulating bodies.

463. I attempted to ascertain this by an immediate experiment; I insulated a wax candle a foot high, ten lines thick, on a glass candlestick; I insulated a tube of glass above the candle, so that the flame entered it; above the said tube I again insulated, with its head downwards, an alembic of brass, the neck of which was bent backwards and forwards several times; yet, out of several very fine threads which I annexed to that neck, none manifested the least electricity, either when the glass tube became deferent by its being heated till it grew red-hot, or when the same afterwards cooled, and thereby recovered its insulating nature.

464. But, of all others, the following observation convinces me most of the above truth. A strong spark from an electric bat-

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tery will vitrify metallic particles, and thus in an instant from *deferent* render them *insulating*; yet no part of the spark is found afterwards to have remained affixed to these bodies; for the deficiency that may have remained on the one side of the battery, after a discharge of the kind we mention, is found to be precisely equal to the remnant of excess left on the opposite side.

C H A P. III.

In which the three properties of electric atmospheres are confirmed by experiments made on convex bodies. Their power of exciting atmospheres of a contrary kind on the same body, is explained; and the point where an equilibrium between those atmospheres takes place, is determined.

465. **T**HE atmospheres of insulating bodies are in one sense immoveable, as well as their electricities; that is, as long as the deficiency or excess of the electric fire, on given points of sticks of sealing-wax, or glass, remain unaltered, the air around them will remain actuated in the same manner; so that the fire inherent in them will remain relaxed, contiguously to the deficient wax, or possessed of a superior tension, contiguously to the exuberant glass; but, in deferent bodies, the electric fire being able to be transfused from the one part of them to the other, the electric atmospheres of such bodies are moveable, as well as the electricities by which they are produced.

466. Now, in conformity to such principle, since the atmosphere of an insulating body never is displaced in consequence of its meeting with the electric fire of a deferent body, it follows, that all displacement that may arise from their mutual action, will take place in the fire of the deferent body; exactly in the same manner as if an elastic finite body strikes against another elastic body, the mass of which is infinite, the infinite resistance of this latter will reject into the former all the motion that may arise from such a shock.

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467. In considering the *electric* alterations that may take place on a deferent body, two parts must be distinguished on the surface of the same, viz. the immersed part, that is to say, that part of its surface which is immersed in the atmosphere of the insulating body placed near it; and the part not immersed, that is to say, that part to which this atmosphere does not reach. The alterations that will take place on these two parts will be contrary to each other in their *quality*; because they will result from a quantity of fire being given (and as it were lent) by the one to the other. With regard to the intensity of such alterations, they will be in a reciprocal ratio to the capacities of the parts on which they respectively take place; so that the deficiency that will arise on that part which will have given its own fire to the other, will be to the excess that will arise on that other part, in the same ratio as the capacity of the latter stands to the capacity of the former. But this principle cannot be thoroughly well understood, till a few additional experiments have been related and described.

468. These experiments I chiefly make with the following apparatus. In a place sufficiently roomy, I insulate four large tubes, or conductors of tin, two inches and a quarter in diameter, the edges of which are coated at their openings with sealing-wax, in order to prevent their drawing, or throwing any electricity; the first tube is eight feet long, the second four feet, the third two feet, and the last only one foot. I have represented all these tubes in one figure (Pl. VIII. fig. 8.) though they were, in my experiments, separated from each other, and they are insulated by silk strings. The surfaces of those conductors are divided by marks into inches, and from every division hangs an electroscope (in the figure they are only expressed at every interval of six inches to avoid a confusion); each of those electroscopes is made with two very thin threads, two inches and an half long, and two very small balls of pith of elder: those electroscopes are so disposed, that the threads hang parallel to each other, and the balls almost touch one another; as the least motion, or shake, will entangle the threads together, care must be taken before beginning the experiment, to separate them.

496. I present the tube of glass, strongly electrified, to one extremity of the conductor A and B, and slowly carry it round the same at the distance of half a foot (a distance through which in dry weather its actuating electricity cannot be communicated to the conductor, and of course lost), and I take care to keep this electrified tube under the first electroscope A, perpendicularly to the axis of the conductor A. I. All the electroscopes between A and 2 B, diverge; the electroscope A most of all, and the others gradually less. The electroscopes between 2 B and 3 B scarcely move. From 3 B to 4 B the electroscopes are also found to diverge, those near 3 B diverge less than those near 4 B. The other electroscopes between 4 B and 8 B diverge all of them almost equally, but sensibly less than the first electroscopes in A. II. The electroscopes in the whole space A 2 B, give signs of a *deficient* electricity; they fly from rubbed sealing-wax. The electroscopes from 3 B to 8 B, give signs of an *excessive* electricity; they fly from rubbed glass, or from the hook of a charged bottle. III. I remove the electrified tube, all the electroscopes fall, and there remains no sign of electricity in any part of the whole conductor.

470. I propose to explain at some length this first experiment; it will greatly facilitate the understanding of the subsequent ones. I. The excessive fire, which continues actuated on the surface of the electrified glass-tube C, actuates an atmosphere in the air around it; it excites a particular kind of tension, or force, in that air, and by its means endeavours to raise a *deficient* electricity on the whole part of the conductor which the said atmosphere surrounds; that is to say, this atmosphere endeavours to drive into the substance of the conductor the natural fire which lies near its surface. II. This power in the atmosphere of the glass-tube gradually diminishes, constantly keeping a certain proportion both with the distance of the said tube, and with the excessive fire which continues to be exerted on the surface of the same; therefore, the atmosphere of this tube will drive into those parts of the conductor which are remoter from the point A, a less quantity of fire than into those which lie nearer the same point; again,

again, therefore, the divergence of the electroscopes must from A towards 2 B gradually lessen, and at last vanish. III. The extremity of the atmosphere of the glass tube, reaches (as we suppose) so far as 4 B; and since the fire which this atmosphere endeavours in all this space A 4 B, to drive into the conductor, cannot be condensed within the substance of the same, it cannot of course enter it without driving an equal quantity of fire out of it, which will become manifested on some other part of the surface of the conductor, which the atmosphere does not reach; for instance, in 4 B 8 B, or even in 3 B 4 B, whereto the atmosphere indeed perhaps reaches, but only in a faint imperfect manner. IV. Hence two contrary forces will result, which will endeavour in contrary ways, to set the electricity in the conductor in motion; the one uneven and decreasing, that is to say, that from the glass tube which, in proportion as the distance of it will be greater, will drive with the less force the fire lying on the surface of the conductor, into the substance of the same; and this fire, as has been observed, must unavoidably drive in its turn an equal quantity of interior fire from some other part of the same conductor. The other force will be uniform; it will be that of this fire thus forced out of the conductor, and kept out on its surface, which will *react* back, and endeavour to force anew out of the body of the conductor a new quantity of inward fire. V. The place where such contrary forces will become equal to each other, will be (and let us call it so) the place of equilibrium: there no visible electricity will obtain, the natural fire of the place being equally solicited there by two equal and contrary forces. VI. Let such place be in the middle between 2 B and 3 B: from the latter point towards 4 B, the divergence by excess of the electroscopes will begin, and keep gradually increasing, according as the atmosphere of the glass-tube gradually ceases to act upon the conductor: in the space between 4 B and 8 B, to which the repressing atmosphere (according to our supposition) cannot reach, the electroscopes will diverge uniformly, in consequence of the excessive fire which, as has been above observed, will be uniformly maintained on that part of the conductor. It indeed should seem, that the electroscopes ought to begin to diverge gradually the one more than the other, from the
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first point A, to the very place of *equilibrium* (which is the middle point between 2 B and 3 B), but really in the whole interval between 2 B and 3 B, no difference between the divergences of the electroscopes can be observed; the opposition of the one force against the other, being in that whole space too inconsiderable to produce any sensible decrease in the divergence of the electroscopes. To the above observations we may add, that the equipoise between the whole uniform pressure that takes place upon the space 4 B 8 B, and the sum of all the unequal pressures effected by the different parts of the unequally efficient atmosphere, on the space A 4 B, may very well be compared to the equipoise by which water raised within a pail by stones, keeps its level, and balances itself with the numerous different pressures effected within it, by the stones which are successively made to raise it to different degrees of height. VII. That afterwards when the electrified tube is removed, all electricity vanishes (no part of the electricity from the glass, we have observed, could get into the conductor) is a plain and necessary circumstance: the whole fire that was kept back from the part of the conductor immersed in the atmosphere, and had retired to the other part upon the surface of which it was kept, falls back into its natural place, as soon as the driving force is removed.

471. I now present the electrified tube perpendicularly under 4 B, that is, under the middle point of the whole conductor A 8 B; when I find that the divergence by deficiency of the electroscopes does not extend farther than a foot and an half on each side of the point 4 B, that is, from 4 B to the middle point between 2 B and 3 B, on the one side, and to the middle point between 5 B and 6 B, on the other. II. I present the electrified tube under the extremity A of the second conductor A 4 B, and the divergence by deficiency does not now extend farther than a foot and an half from A; that is, than the middle point between 1 B 2 B.

472. I. I now present the tube under the middle point 2 B of the same second conductor A 4 B, when I find that the divergence by deficiency of the electroscopes only extends to one foot; that is, to 1 B on the one side, and to 3 B on the other. II. I present the tube under the extremity A of the third conductor A 2 B,
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and the divergence by deficiency only extends to one foot, from A to 1 B.

473. I present the tube under the middle point of the same third conductor A 2 B; and the divergence by deficiency of the electrosopes does not reach even to one half part of an interval. II. I present the same under the extremity A of the fourth conductor A 1 B; and I likewise find that the divergence by deficiency does not extend even to an half part of the length of the conductor. But in all these different operations I find, that in order to produce the same degree of divergence in the first electroscope A of each conductor, the tube must be brought nearer to the conductor, according as the latter is shorter.

474. The reason of this is, that when the electrified tube is brought equally near to the extremities A of all the conductors indiscriminately, its atmosphere occupies a proportionally greater part of those conductors which are shorter, and also occupies the similar parts of them with proportionally more efficient parts of itself. Therefore, in the shortest conductors, the electric atmosphere produces with its outmost parts, a resistance proportionably superior to the effect it endeavours to produce with its first parts. With its first parts it endeavours to drive into the substance of the conductor the natural fire which is diffused over the surface of the same near the extremity A; but then its actually producing such an effect, intirely depends on the possibility there is, both that an equal quantity of natural fire may be driven from the inside of the conductor, and that it may accumulate on the surface of it near B. Now, as those successive parts of the atmosphere of the glass tube which extend to the extremity B, are the more efficient in proportion as the conductors are shorter, they repel the more strongly the fire which the first parts of this same atmosphere endeavours to make spring out of the spot on which they act; and, therefore, oppose the more the immediate effect which that first part endeavours to produce in the vicinity of A.

475. Let, therefore, the tube be brought nearer to the shorter conductors; it is true their extremity near B, will, by that means, be immersed in a more efficient part of the atmosphere than before; but since the efficiency of such atmosphere, undoubtedly increases
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after a greater ratio than the simple inverse ratio of the distance of the center of the said atmosphere, the consequence of the tube being thus brought nearer, will be, that the efficiency of that part of the atmosphere which will surround the conductor near A, will have increased in a much greater proportion than the efficiency of that part which will surround B, can have done; the effect of the bringing the tube nearer to the conductor, will therefore cause the efficiency of the first parts of the atmosphere to surmount the sooner the resistance opposed by the following parts of it; this efficiency will be able to make the electric fire spring from such places of the shorter conductors, for instance, from B, as can yet give no fire, or very little of it, in longer conductors, or even still admit the fire which is driven into them. In consequence, therefore, of the tube being placed nearer the conductors, in proportion as they are shorter, the place of *equilibrium* will draw proportionally nearer to A; and the two series of contrary divergences, that of divergence by deficiency, and that of divergence by excess, will converge the more rapidly, and with the more exactness, towards this point of equilibrium.

If it were possible to bring the above experiments to a sufficient degree of accuracy, a vast field would be opened here for Geometry to exercise itself. But I have proceeded as far, I think, as the subject of this work can admit.

476. From what has been said, every body surely understands, that if the conductor be still more shortened, and every way lessened, it will at last be wholly surrounded by parts of an electric atmosphere, that will be very nearly equal to each other in point of efficiency; so that as long as it will remain insulated, no sensible excess or deficiency will be manifested around it. This is the case with a stripe of gold-leaf insulated with a very dry thread of silk, and immersed, for instance, in the atmosphere of the Chain: the fire within it remains unmoved, and we may say equally repelled from every side; and it is only when I present my finger to it, that the tension of the excessive fire of the atmosphere, drives the natural fire of the stripe towards the finger, where such tension is weakened by it.

477. But

477. But if the body immersed in the atmosphere be of any considerable length and annexed to an insulating body, so that this atmosphere may give some fire to it, or draw some fire from it, such body will move, be attracted, and then repelled, as often as it will be presented to the electrified body that actuates this atmosphere. To explain this, I will alledge the example of the threads of the scrutator E (Pl. VII. fig. 1.) I present them obliquely to a conductor (Y, Pl. I. fig. 1.) and they are attracted by it: this is because the atmosphere of the conductor can then drive some natural fire from the extremities of the threads which are presented to it, towards the other extremities which are contiguous to the sealing-wax. I now present the same threads to the conductor in a lateral situation still, but somewhat underneath, and they are repelled by it; the reason of this repulsion is, that as the lump of sealing-wax has become, in the present situation of the scrutator, immersed in the atmosphere of the conductor, some fire is driven from it by that atmosphere; into those parts of the threads which are near the wax, and they become in consequence of this, electric by excess.

478. Since I have mentioned here the scrutator, I shall by its means examine into some other seeming irregularities (in reality there is no irregularity, their appearance can only proceed from our ignorance) that take place in some extremely complicated motions of the threads of that instrument, especially when I use it in order to try the electricity of the electric well. I. Let the well A (Pl. VII. fig. 5.) be electrified by excess, and the scrutator not be electrified at all. If I lower the latter along the axis or middle of the well, the two curves, expressed in the figure, indicate the line traced by the stripes of paper in their way down the well; that is to say, when those stripes begin to be immersed in the atmosphere of the well, they begin with diverging, because the atmosphere of the well then drives the natural fire of the threads up to the sealing-wax. But as the threads are immersed lower and within a gradually more active part of the atmosphere, their divergence increases; if they are lowered still more, they manifest a kind of tendency to move upwards, as if drawn by the electricity of the upper part of the well; but so soon as they are

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carried to a certain degree of depth, they fall: the reason of all these accidents is, that the atmosphere which is diffused around the sides of the well, is now enabled by the situation of the threads to drive downwards the excess that has gathered near the sealing-wax; therefore, as soon as that excess has returned to its former place, the threads fall, and continue to descend so far as to the bottom of the well, without any farther motion or divergence.

479. Let both the threads of the scrutator and the well be electrified alike, for instance, by excess (Pl. VII. fig. 6.). Usually in the act of immersion into the atmosphere of the well, the threads are repelled and thrown back upwards, as being similarly electrified with the well; but when they come to be actually immersed into the well itself, they are somewhat attracted by its sides, because the atmosphere of the well being very strong, introduces a change into the situation of the fire in those threads, which it drives up to the wax. At a certain inconsiderable depth in the well, the excess from the sides so disposes and equilibrates the excess in the threads, that no divergence takes place. Below such place, the threads resume the same divergence which they had in consequence of their own excess before they entered either the well or its atmosphere, because they then have reached a place where no atmosphere is exerted.

480. Let the threads of the scrutator and the well be contrarily electrified. When the threads begin to be immersed into the contrary atmosphere of the well, they diverge from each other, and manifest a kind of tension; when they are immersed into the well itself, they are laterally attracted by the sides of it, consequently diverge to a very great degree. Lastly, in the bottom of the well, where no atmosphere besides their own is exerted, they resume that degree of divergence which they derive from their own electricity.

481. But let us return to our experiments with the conductors mentioned above. Before I present the rubbed glass under the extremity A of the conductor A B, I take care to introduce a small excess into the latter, and all the electroscopes diverge accordingly. Approaching then the rubbed tube to this conductor,

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I observe that the first electroscope in A loses its whole divergence by excess; then the second also loses its own; then the third, then the fourth, &c. so far for instance as the mark 1 B, to which the place of the equilibrium is brought in this experiment, whereas it formerly stood farther, between 2 B and 3 B. Then the first electroscopes begin again to diverge, but by deficiency; and the divergence by excess of the remotest ones, at the same time increases.

482. If I experiment after the same manner with the other lesser conductors a 4 B, a 2 B, a 1 B; that is, if I present under them the rubbed tube, after I have introduced into them some excess, the same effects result in them that I have above described; in all, the electroscopes in the vicinity of a , at last manifest a divergence by deficiency; in all, the *place of equilibrium* is found to be nearer the extremity a than it was before; and, in all, the electricity by excess of the farthestmost electroscopes is increased; but with this circumstance which must be taken notice of, viz. that when the conductors are shorter, the rubbed tube must, in order to produce the above effects, be brought nearer to a .

483. But if I make afterwards one of the above conductors communicate with the Chain, animated with a strong electricity, then the glass tube can no longer destroy the divergence by excess of the nearest electroscopes, much less introduce at last into them an electricity by deficiency and then attract them; but the strong active excess which is now exerted around those electroscopes, maintains its ground, and their threads continue to fly from the glass tube.

484. All the experiments above described must indeed appear surprising, if we consider that they exhibit an instance of two contrary electricities combined together, as well as of a place where there is none, on one and the same body. But our surprize will increase still more, if we carry farther our experiments on this subject, since we shall find, that we may both draw fire from one part of a body which has only its natural quantity, or even is deficient, and give this fire to the other part of the same body, which has more than its natural quantity. In fact, I. If while I hold the rubbed tube under one of the conductors, a person presents his finger

either to the place where the electroscopes diverge by excess, or to that where they do not diverge, or also to that where they diverge by deficiency; in all those places he will draw off a spark. II. In consequence of this drawing off a spark, the divergence by deficiency of the electroscopes in the vicinity of A will increase, and be extended to some farther electroscopes beyond them; but all divergence by excess in the farthestmost electroscopes will cease. III. And if in such state of things I remove the tube, a small divergence by deficiency immediately takes place in all the electroscopes of the whole conductor.

485. The above accidents may be easily understood, if we consider, that the excess which, in consequence of the pressure of the atmosphere of the rubbed tube, is maintained on the surface of the conductor beyond the place of equilibrium, solicits every particle of fire within that part to spring out; and as the finger, wherever it be presented, lessens the resistance from the ambient air, which keeps (as we shall see more explicitly in the following chapter) such fire from actually springing out, it follows, that a certain quantity of it must now be thrown from the conductor, &c.

486. If all the above experiments are repeated by presenting a stick of sealing-wax, or an oiled stick, to the conductor, instead of a glass tube, all the same phenomena will take place, but with contrary motions of the electric fire. I. The deficiency excited on the surface of such sticks will excite a deficient atmosphere in the ambient air. Hence the natural fire diffused in it will grow relaxed around the surface of the sticks; it will raise an excessive electricity on the nearest part of the conductor to which it is presented, and cause the fire diffused on the farthestmost surface of it, to flow back, and come to accumulate itself on the said nearest part. II. Thence will result on this nearest part a divergence by excess; in the farthestmost part, a divergence by deficiency; and, in its proper place, an equilibrium. III. The stick being kept under the extremity A of the conductor, and a finger being presented to any part of it, a spark will be thrown into the conductor. The cause of this is, first, that every particle of natural fire within the conductor, is solicited to move towards

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the farthestmost part of it, in order to fill up its deficiency. The property of all deferent bodies, of constantly preserving the same quantity of fire, prevents any fire from rushing into the conductor; but when a strange body is presented to this conductor, as much fire runs to it, as can be admitted; and this fire supplies the deficiency at the other end of the conductor. IV. Therefore, the divergences on the farthestmost part of the conductor, will be suppressed in the same manner as those by excess were, in the former experiment; the contrary divergences on the nearest parts of the conductor will increase also in a like manner, in a like manner the *place of equilibrium* will change, &c.

487. We may observe, that these experiments made on *unmoved* electric atmospheres, applied or presented to deferent bodies, may be varied *ad infinitum*. I. By varying the shape and the capacity of these bodies; to which they are applied. II. By varying the nature of the *unmoved* atmosphere, since it may be actuated either by an *excessive*, or a *deficient* electricity. III. By varying the state of the same deferent bodies; thus they may be electrified before hand, either by excess, or by deficiency, more or less strongly. Again, if they have no electricity, they may or may not be insulated. IV. By varying the distance at which the unmoveable atmosphere is presented to them. By applying either jointly or successively, two or more such *unmoved* atmospheres, whether homologous or contrary, to divers places of those bodies. But every body may see, that to dwell on all these different complicated cases would be infinite, and even superfluous, since the simple ones above explained may afford a solution for them all.

488. Nay, from the above facts and reasonings, we may foresee what the case would be, if two atmospheres both *moveable*, were made to unite together. I take a tube made of tin, insulated on a long handle of sealing-wax (Pl. V. fig. 1.) equal in every respect to the part A 3 B (Pl. VIII. fig. 8.) of the whole conductor A 8 B, let us therefore call it *a 3 b*. I electrify it by excess with the hook of a bottle, and place it near the part A 3 B of the conductor A 8 B, so that their sides are parallel, and only three inches distant from each other. I. The electroscopes in A 3 B, along the side of *a 3 b*, manifest a divergence by deficiency. II. This divergence by deficiency soon lessens, and then vanishes
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in all that part of the conductor A 8 B, which lies beyond A 3 B; that is, beyond that part to which *a 3 b* corresponds. III. Then the electroscopes in 4 B begin to shew a divergence by excess; and the following ones in 5 B, 6 B, 7 B, 8 B, likewise manifest an excessive electricity, which is somewhat greater than that manifested by the former, and besides is uniform throughout.

489. These phenomena are of the same kind as those which take place when the *unmoveable* atmosphere of the rubbed glass tube is brought under the extremity A of the conductor A 8 B, and have a similar cause. I. The moveable excessive atmosphere of the conductor *a 3 B*, drives the natural fire from the surface of the other conductor A 8 B into the substance of the same, and forces an equal quantity of fire to issue and keep out on its surface, in the part 4 B 8 B. II. The action of such excess against the pressure of the atmosphere, produces, in a certain place somewhere beyond 4 B 8 B, a place of *equilibrium*, &c.

490. There are, however, a few differences between the latter experiment and the former ones, which must be taken notice of. I. The *moveable* atmosphere of the conductor *a 3 b*, remains applied to A 3 B, by parts of itself which are equally distant from the same *a 3 B*, that is to say from the body by which it is actuated, and which are therefore equally efficient throughout, so that it seems that an uniform deficiency ought to take place all over A 3 B; but as the several parts of the atmosphere of *a 3 B*, co-operate and act with more force about the middle of it than at either of its extremities, they create both a greater divergence, and a greater attraction in the electroscopes situated in that part, than in those at the extremities. II. The moveable atmosphere of the conductor *a 3 b*, ought, it seems, in consequence of its parallel position, exert a very great efficiency in driving the natural fire from the correspondent surface of A 3 B; but yet, as it of itself possesses much less activity than the rubbed glass tube, it cannot reach to so great a distance as the latter does; hence the place of the equilibrium stands no farther than between 3 B and 4 B, very near the extremity 3 *b* of the conductor *a 3 b*; the divergences by excess from 4 B to 8 B, are also less considerable, &c.

491. If I previously introduce a certain degree of excess into A 8 B, I find, that the divergences on its part A 3 B, are less,
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when the conductor $a\ 3\ b$ comes to be presented to it, than they were before; the *place of the equilibrium* lies still nearer to $a\ 3\ B$, and the divergences by excess in $4\ B\ 8\ B$ are greater than formerly. The reason is, that the excess previously introduced into the whole conductor $A\ 8\ B$, prevents so great a deficiency as that before from taking place in the part of it $A\ 3\ B$; and, in the meanwhile, as the sum of the excess in $4\ B$, now consists both of the excess that has been introduced into it at first, and of that which has been driven into it from $A\ 3\ B$, it, of course, proves greater in this experiment than it did in the former.

492. If the conductor $a\ 3\ b$ be placed near the conductor $A\ 3\ B$, when a deficiency has been introduced into the latter, the divergences by deficiency will become greater; the deviation of the electroscopes towards the conductor $a\ 3\ b$ will likewise be greater; the place of the equilibrium will be removed farther off beyond $3\ B$, and the divergences by excess in $4\ B\ 8\ B$ will be proportionally less than before, or even there may be none.

493. If in each of these cases the conductor $a\ 3\ b$ be moved along $A\ 3\ B$, keeping all the while its parallel position with respect to it, the divergences by deficiency will follow the conductor $a\ 3\ b$ in its progress, and at each end of it there will now be a distinct place of *equilibrium*, which will be accompanied by a place of divergence by excess.

494. If, in each of the above experiments, the electricity of the conductor $a\ 3\ b$, or also that in the conductor $A\ 3\ B$, be inverted, the same phenomena will take place, but after a contrary manner, conformably to what has been explained above.

495. Universally; since, when an atmosphere of a given kind is presented to a body animated by an electricity of the same kind, the electroscopes annexed to that body both lose their divergence, and tend to acquire a contrary one, so, when an atmosphere of a given kind is presented to a body impregnated with a contrary electricity to its own, the electroscopes of the latter tend to increase their former divergence, or tension; because, in this case, the atmosphere of the body we suppose, endeavours to increase that contrary electricity with which it now meets.

496. I say divergence, or tension: in order to explain this, I must remind the reader of this principle, that *bodies similarly electrified repel each other, and bodies contrarily electrified attract each other*. Therefore, when I present the rubbed tube to the electroscope A of the conductor A & B, which is electrified in the same manner with it, the first motion of this electroscope is always to fly back; though, it is to be observed, such motion is shorter in proportion as the electricity previously introduced into A & B is less, and as the capacity of this same A & B, beyond the limits of the atmosphere of the rubbed tube is greater; the reason is, that in the latter case, the electricity in A, of the conductor A & B, which occasions the above motion, is the sooner dissipated by the atmosphere of the glass tube, and the sooner also turned into a deficiency; so that the electroscope may be already attracted, nay, may be already electrified by deficiency, though it has not had sufficient time to appear sensibly repelled, and to fall gradually when losing its divergence by excess: in order, therefore, to be able to discern the instant of such a material change as that which takes place in this electroscope, the rubbed tube must be presented to the conductor A & B, from a considerable distance, and very slowly. Again, if the weather be very favourable and dry, and if the electroscopes are made with very dry threads, a new accident will take place, of which I must inform the reader, which is that the cork-balls will at first fly to the conductor; the reason is, because the rubbed tube will have introduced a deficiency into the very *deferent* conductor, before the dry threads can have had time to lose their own excess; whence the cork-balls, being still electrified by excess, will at first run to the conductor, which has already become so by deficiency.

497. Now, with regard to the tension of the threads, which I began to mention, I observe, that as soon as this tension takes place, it is a sure sign of their electricity being turned into a contrary one. As long as there remains in the electroscopes any electricity homologous to that which produces the atmosphere around them, the said electroscopes proportionally fly back (they always
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fly back when their own homologous electricity cannot be driven away, either on account of its intensity, or for want of a capacity into which it may lose itself) and also proportionally diverge. Afterwards, in the instant when there remains no electricity whatever in the electroscopes, the threads of the same naturally hang somewhat shrunk in and contracted, as hanging threads generally do. But, lastly, when the surrounding atmosphere now begins to introduce into the electroscopes an electricity contrary to that which they had at first, then the threads begin to be proportionally directed to the body, from which this atmosphere springs; and they do this with such a degree of tension, as makes them lose their tortuous appearance. Whence it happens, that they indeed both diverge from each other, in consequence of their being again animated by the same kind of electricity; but, it is to be observed, that this divergence is lessened by their common tendency towards the rubbed glass, which now attracts them; so that the whole intensity of their electricity is not to be estimated from the degree of their divergence only, but in order to value it with accuracy, account must also be had of the said tendency.

498. Every one who knows how important it is in experiments on *natural*, and much more on *artificial*, Electricity, accurately to distinguish the different changes of an electricity into a contrary one, will not, I am certain, think that I dwell here upon too minute objects. However, I now proceed to deduce, from what has been said on the reciprocal actions of electric atmospheres, a more exact determination of a principle, which is of the greatest importance in Electricity; I call it *the principle of, both the equilibrium, and the motion, of the electric fire.*

C H A P. IV.

On the duration of electric atmospheres, and on the state of electricity in air which is continually renewed. More complete principles with respect to the equilibrium and the motion of the fire. On the general motion of the electric fire contained in a body, towards that place whence a spark is drawn; and on a certain curvature which takes place in this spark, in consequence of this same motion, the law of which is ascertained.

499. **D**R. Franklin has often found, that after turning round over his head, more than one hundred times, an electrified cork-ball, the same still possessed a degree of electricity. Mr. Kinnerley (Franklin, p. 384.) communicated some electricity to another person, by throwing him his hat. I have often tried to electrify with a glass tube those long iron wires with which I use to examine the electrical state of the atmosphere, when the state of the latter was such, that they derived of themselves very little, if any, electricity from it; and always found, that in serene and windy days, they preserved their electricity as long as in serene and calm weather.

500. Now, this may appear a kind of paradox, that in air, which perhaps is renewed a thousand times, the same attractions and repulsions should continue to take place, as do in unchanged air; but the nature of electric atmospheres, which we have before explained, likewise explains the wonder of this phenomenon. The electricity of a body resides within the superficial pores of it, and actuates the ambient air, not by diffusing itself into it, but by exciting either a tension or a relaxation in the natural fire inherent in it; therefore, that air, as soon as it is left (through the progressive motion of the electrified body) behind the limit to which the action of the latter extends, recovers its natural state: the air which takes its place, in its turn experiences a tension (or a relaxation) in its own natural fire, in the same manner as the former did; and thus both *the actuating electricity*

city in the body, and the actuated atmosphere in the ambient air, continue to remain the same, in the same manner precisely as would happen, if the same identical air continued around the electrified body.

501. But this proposition with regard to the duration both of electricities, and electric atmospheres, in renewed air, will be rendered more simple, by confining it to its true limitations. The first exception to the above proposition is, that when the electricity raised in a body is extremely strong and intense, then indeed this electricity, as well as the atmosphere of that body, do not last so long in continually changed, as they would in calm, permanent, air. I charge two equal bottles at once, till the charge gushes out of the hook of both, then I make one of them whirl round over my head with great rapidity; and, I find, that the strong exuberant excess in it ceases somewhat sooner than in the other bottle which I left unmoved. The reason is, that, as the excess which dissipates from the unmoved bottle continues to meet with the same permanent air, it cannot but be in some degree retarded; whereas the excess which dissipates from the whirled bottle, springs out of it somewhat more readily, as it continually meets with air free from any excess. The other exception is, that if the ambient air is not of itself insulated with a sufficient degree of exactness, the mixture of a new quantity of it, will lessen the electricity of the bodies within it. Thus, if I open both the window and door of my room, when the weather is damp, the electricity of my apparatus, however weak it may already have grown, will abate still more in consequence of this introduction of exterior and new air into the room; the additional vapour brought in, in the latter case, must necessarily carry away a quantity of electricity, besides that which has been before dissipated.

502. However, we may here observe, that the continuation of the electricity of a body in renewed air, (under the above limitations) though it has hitherto been one among the innumerable mysteries of nature, is now demonstrated to be no more than a consequence of what has been above explained, both concerning *actuating* electricities, and the manner in which they operate on the atmospheres of bodies.

503. Availing myself of the above knowledge, I shall now proceed to lay down *a principle respecting both the equilibrium and the motion of the electric fire*, more comprehensive than that which I used in former places to propose. Hitherto I have used to take notice only of the expansive force of that electric fire which lies diffused on the surface of bodies, but now I propose also to take into consideration the action which results from the tension of the electric fire in the air. In fact, *such tension (or action) of the electric fire inherent in the air, is, of itself, sufficient to alter the equilibrium of the fire in the bodies within it, and always contributes either to form, or to restore, this equilibrium.*

504. When I present a rubbed glass tube to the head A (Pl. VIII. fig. 8.) of the brass conductor A B, it is only the excessive tension of the electric fire inherent in the air that surrounds the rubbed tube, that drives the natural fire from the extremity A of the conductor, towards the extremity B. When I present to the same conductor a rubbed stick of sealing-wax, then it is the relaxation of the fire inherent in the ambient air, that makes the natural fire in the conductor flow back from B towards A; therefore, a simple alteration in the tension of the electric fire in the air (whether the same be increased or lessened), will alter the equilibrium of the fire inherent in those bodies which are immersed in it.

505. And this same tension, as long as it remains in its natural degree, contributes to produce the same equilibrium of the electric fluid; because, since the alteration of this tension alters the balance of the electric fluid, and the alteration of this balance reciprocally alters such tension, it follows that the equilibrium that takes place in the fire proper to bodies, whenever it obtains, must be in a great measure preserved by the fire inherent in the ambient air around them, when the latter likewise possesses its natural degree of tension.

506. That, afterwards, when the natural equilibrium of the fire in bodies happens to be altered, it may be restored by the tension of the fire in the air, the degree of which is likewise altered, is what manifestly results from every experiment. When I touch an electrified body, I take from the same both its actuating electricity

tricity and its atmosphere; therefore, the positive or negative force of such atmosphere has been employed, either in expelling the excessive fire, or in supplying the deficient one; that is to say, the excessive tension of the fire proper to the air around that body, has been employed in driving off the fire exuberating in it; or in the case of a negative electricity, the relaxation, or deficient tension, of the fire in the air around the deficient body, has facilitated the introduction of new fire into it.

507. But here is another experiment, with which I usually also prove the above mentioned functions of electric atmospheres. With a bit of wax I fasten a needle on the tube A 3 B (near 2 B) perpendicularly to it; then I move the glass tube, strongly electrified, backwards and forwards in the vicinity of the needle; this done, I carry the rubbed tube near the conductor, and parallel to it; when a spark uncommonly strong is thrown out. If the operation be made by two persons, and two rubbed tubes be thus presented to the conductor, the spark will be still stronger, and with regard to its crack and light much resemble that of the discharge of a bottle.

508. Now, if *strange* atmospheres can thus produce such uncommon sparks, surely the atmosphere itself proper to a body, must also contribute to excite sparks from it. The concurrence of the atmospheres of the above rubbed tubes, increases the tension of the air around the conductor to such a degree, that besides the exuberant fire, it moreover drives the natural fire out of it; and in fact, when, after exciting the spark, the tubes are removed, the conductor is found electric by deficiency. If, instead of tubes of glass, sticks of sealing-wax are used, the re-union of their atmospheres so much increases the relaxation of the fire in the air around the conductor; that, besides the deficient fire, even excessive one runs to its surface, so that, the sticks being removed, the tube is found electric by deficiency. Therefore, we are well grounded to say, that the tension alone which arises in the fire proper to the air around a body, by virtue of the fire which exuberates in that body, contributes to repel and repress this same exuberant fire: and the relaxation alone, which takes place in the fire proper to the air around a body, by virtue of the deficiency which

which takes place in it, contributes to determine towards it a proportional quantity of fire to supply that deficiency.

509. Nor is there any danger that atmospheres proper to bodies will not operate in the same manner as *strange* ones, since the tension of the fire proper to the air around a body, operates from a distant circumference towards the surface of that body, with a force equal to that with which it would operate from the surface itself of the body, towards the said circumference, conformably to the universal law of equality between actions and re-actions. The above manifestly agrees with experiments; when I present the rubbed glass, or sealing-wax, under the extremity A (Pl. VIII. fig. 8.) of the conductor A 8 B, then the atmosphere which takes place near the extremity 8 B, is balanced by that which surrounds the extremity A; therefore, the latter atmosphere operates against the conductor, in a direction that is from the surface of the rubbed glass, or wax, towards its own circumference; and the atmosphere proper to the conductor balances the other atmosphere, by operating in a contrary direction to it; that is, from its own circumference, towards the surface of the conductor.

510. Having thus demonstrated this truth, that the force of the fire proper to the air around bodies, jointly concurs with the force of the fire proper to those bodies themselves, in creating the motions of the electric fluid, it now remains that we should bring this truth to its proper universality; because, it is not only in the electricity of a simple excess, or a simple deficiency, that the absolute tension or relaxation of the fire in the ambient air is to be considered, but in all kinds of electricity, universally, (93) the overplus of tension in the air around one body above the tension in the air around another body, will contribute to the greater efficiency of the former above the latter's, in determining the motion of the electric fire; so that, I. If we treat of an electricity of an absolute excess, such, for instance, as that in the Chain with regard to the ground, then we may say, the fire that exuberates in the Chain, is determined to pass into the grounds in consequence of the superiority of its expansive force above that of the fire in the ground, and also by the superiority

rity of the tension in the air around it, above that which obtains in the air contiguous to the ground. II. If we treat of an electricity of an absolute deficiency, such as that, for instance, which takes place in the Machine relatively to the ground, then we ought to say, the natural fire in the ground will be determined to flow into the Machine, in consequence of the superiority of its expansive force above that of the defective fire in the Machine, and also by the superiority of the tension in the air contiguous to it, above that in the air contiguous to the deficient Machine. III. If we treat of a *respective* electricity by either excess, or deficiency, then we are to give the same explanations with regard to the particular bodies in which such electricities take place, as have been given in the cases of the Chain relatively to the ground, or of the ground relatively to the Machine. IV. If we treat of a *respective* electricity *by excess and deficiency together*, then we ought to say, the superiority in point of expansive force, and of tension, that takes place, in and around, the one of the two bodies, will increase in consequence of both the excess of fire in that body, and the deficiency of it in the other. V. If we treat of *respective* electricities either of *unequal excess*, or of *unequal deficiency*, then we are to say, the force which produces the motion of the electric fire will lessen, because the superiority of expansive force, and tension, in and around the one of the bodies, will lessen proportionably to the expansive force and tension in and around the other body.

511. Thus it will be universally understood, how the motion of the electric fire between two systems, or bodies whatever, is determined, both by the overplus of the expansive force in the fire on the surface of the one of them, above that of the fire on the surface of the other, and by the superiority of the tension in the air around the former, above the tension in the air around the latter.

512. The *respective* excess of the tension in the air around a body A, also contributes to the motion of the electric fire which, in any degree, superabounds in that body, by determining it to flow towards that part of it, where this tension becomes weakened, in consequence of the approach of another body B, which is, in any degree defective with regard to the body A. Thus, when

when I present my finger to the Chain, the excessive fire of the Chain drives off the natural fire from the surface of my finger, and thus introduces a kind of very small charge over it: now, this driven fire is like a support that is taken away from both the excessive fire in the Chain, and that in the intervening air; therefore, the excessive fire around the whole Chain, drives a somewhat greater excess towards the place where I approach my finger, then towards any other part of the Chain. When I present my finger to the Machine, then, I say, the natural fire that is *wanting* on its surface, is like a support taken away from the natural fire of the intervening air; therefore, the natural tension of the electric fire in the air that surrounds my body, drives the natural fire from the whole surface of it, and makes it flow towards my finger; then this fire, in its turn, drives a new portion of fire from the part of the Machine which lies nearest to it, and thus increases the deficiency in this same part; which must proportionally fill up and lessen the same in all the others.

513. With regard to this *flowing* of the fire of an electrified system, towards that part of it to which another system, or body, is brought near, I should perhaps fear lest I have been somewhat too diffuse, if I were not convinced of the importance of the subject, and of the great light which the experiments I have related procure to the science of Electricity. I even shall add another, which is simple, and, in my opinion, decisive. When your electroscope of the Chain, diverges a little, in consequence of a very small degree of electricity being diffused into the latter, approach slowly to it, the back of your hand, and you will see the balls of the electroscope, which were hardly separated, recede from each other more than to three lines distance; especially if your conductor be very large, such as that in fig. 1. Pl. I: in order to produce the above small electricity, you only need approach the rubbed glass tube to the conductor, and by keeping it at a certain distance from the conductor, you will be able to raise as small an electricity as you please.

514. The above is to be explained in the following manner: the tension of the fire in the air around the conductor, where it has its natural support, makes the small excess in the conductor, run
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to the place to which the hand stands near, and deprives the fire in the air of its support, in consequence of the free passage that obtains through the hand into the ground. If the conductor be electrified by deficiency, in this case a certain quantity of excessive fire will run to the above part of the hand, and to the air contiguous to it, in consequence of the tension in this air being now increased; which excessive fire will, in its turn, drive a farther portion of the natural fire remaining in the conductor to another part of it, when the tension of the natural fire in the air around it, is deficient and below the natural degree.

515. Now this *flowing*, just now described, of the excessive fire of a body to some one part of the same, under certain circumstances, this *flowing*, I say, when it is effected with a certain degree of force, causes this fire, in that place whereto it is determined by the approach of a strange body, to divide the intervening air, in the very same manner as a strong spark sometimes breaks through a sheet of glass that opposes its free passage. In regard to this fact, I have, besides, another experiment which confirms both the reality of the above *flowing* of the excessive fire, and its power of determining, according to a certain settled law, the direction of the spark that is thrown out in consequence of it, which law, as far as I know, has not been mentioned by other Writers.

516. I take a brass rod A B (Pl. VIII. fig. 9.), which is solid and half an inch thick, and terminated in A, into an hemisphere; I present it to the conductor Y (Pl. I. fig. 1.) animated by a strong electricity, and bring it near the same in a situation almost parallel to it, in such a manner that its rounded end be the nearest to the conductor; when it constantly happens that the spark flies to the head of the brass rod in an incurvated direction, the cavity of which is constantly turned towards that part of the conductor along which the rod is placed. Likewise, when I present my finger to the conductor, after the same manner as I did the brass rod A B, the same effect still obtains; that is, the spark flies to the tip of my finger in an incurvated direction, the cavity of which is turned towards my finger. In short, the person who experiments has it in his power to make the spark bend its way in any given direction;

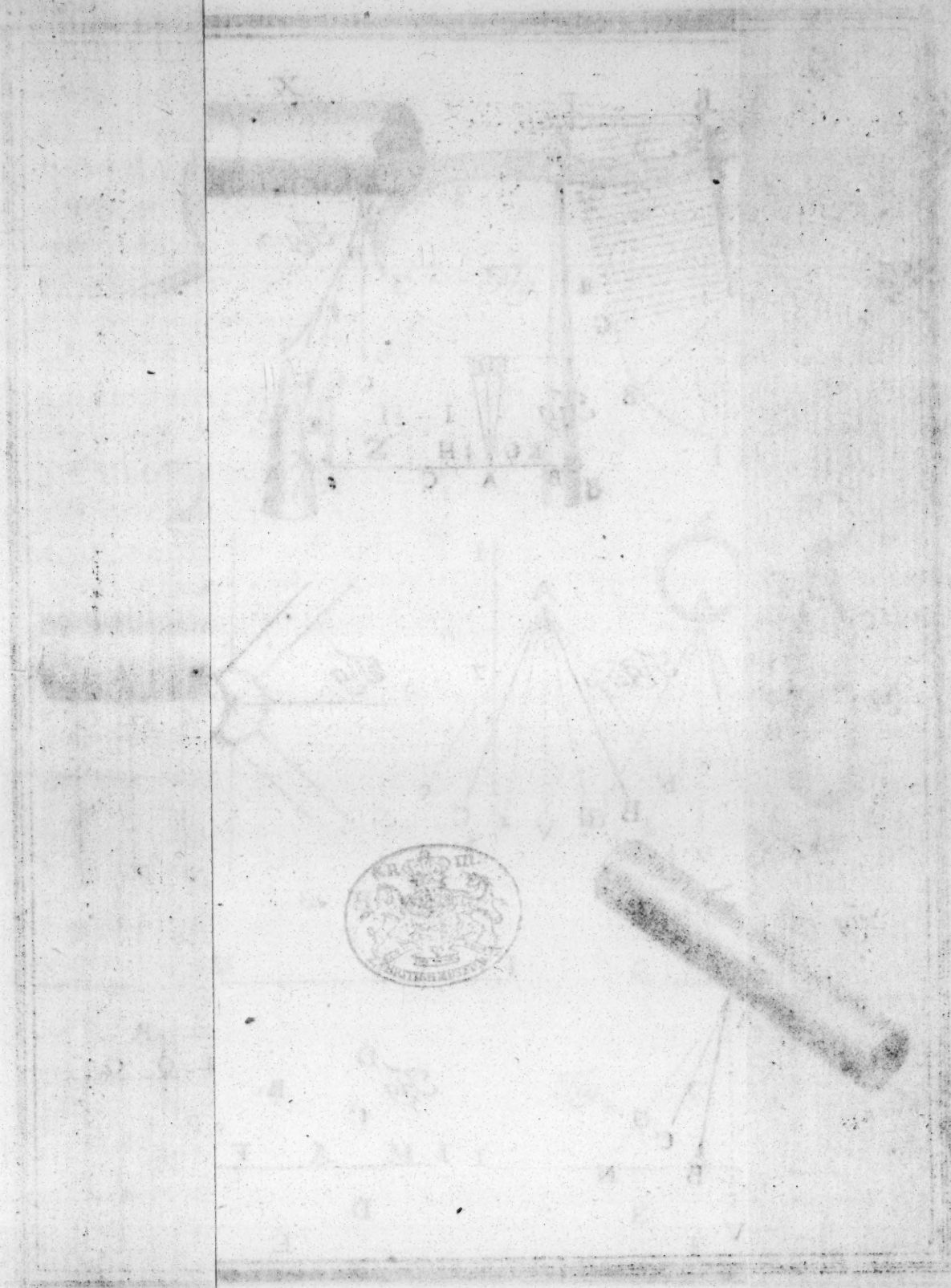
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only,

only, it is necessary to excite the spark from such parts of the conductor as are free from asperities.

417. The above effect is to be explained thus; the *wave* of excessive fire from which the spark results, is determined by the superior tension of the fire in the air around the conductor, to flow to D (a place where the stratum of the intervening air is lessened, and consequently resists less) along the side CD of the conductor, collaterally to which the tension of the fire in the air grows continually less and less supported, in consequence of the fire which this *wave* becomes enabled to drive into the ground, both from the other surface of the same air, and from the brass rod. Therefore, if this direction of the wave is only considered, we shall find, that it must proceed directly towards E; but with such direction we are to compound the force which will result from the resistance of the stratum of air through which the spark is to break; and this resistance, by gradually turning the spark out of its way, will make it bend its course towards the end A of the brass rod.

518. But here I find that the *pressing* electricity of electric atmospheres, has led us, in a manner unawares, to consider the vivid electricity of sparks; a subject which is as extensive as it is important, and must be treated in a section apart.



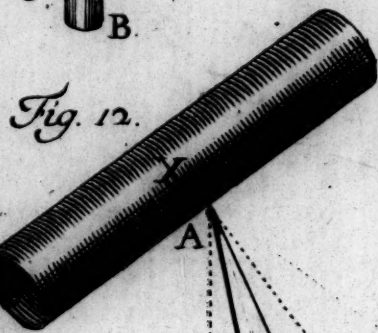
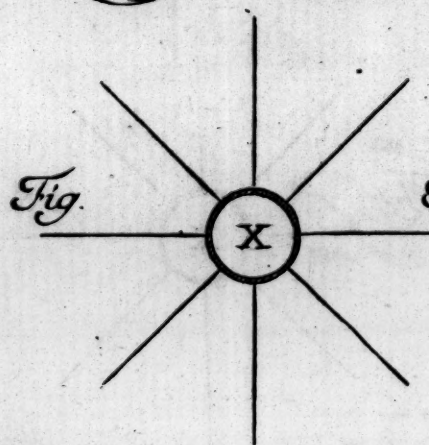
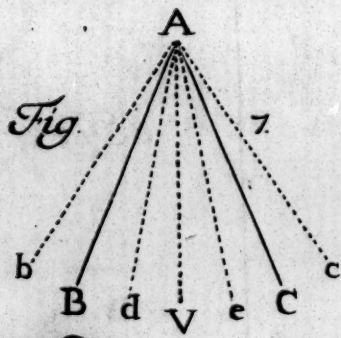
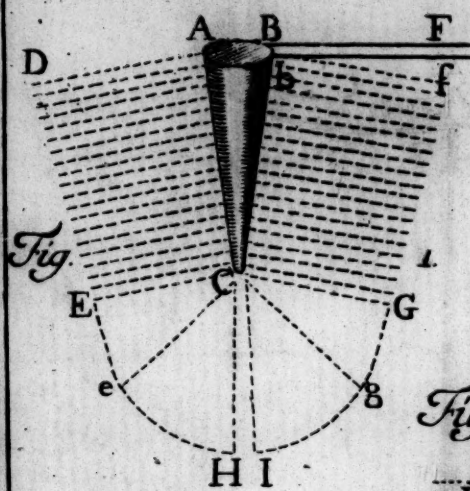
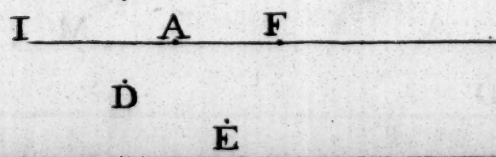


Fig. B 13.



Fig. D B 13.



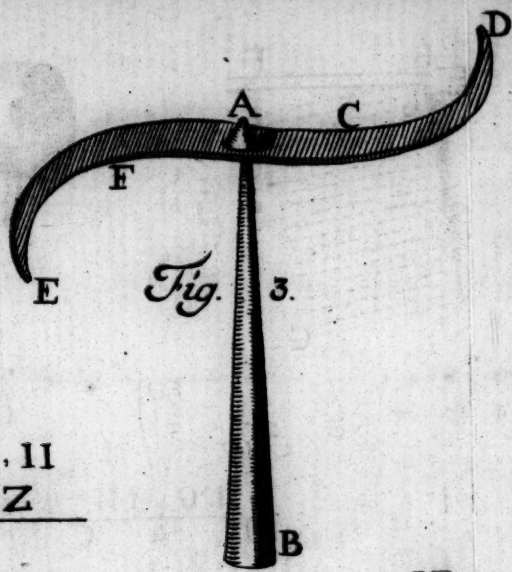


Fig. 3.

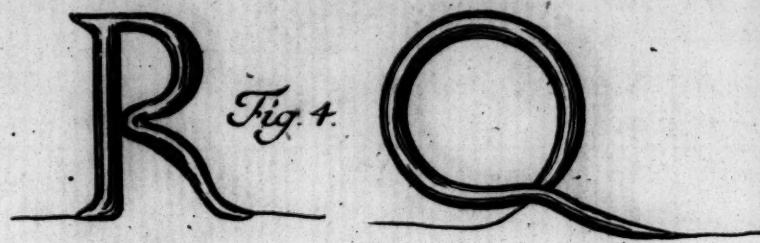


Fig. 4.

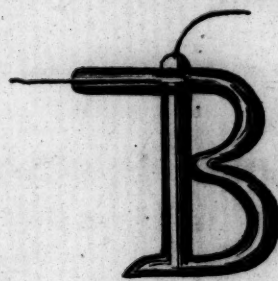


Fig. 5.



Fig. 10.



A	C	E	G	I	M	O	Q
B	D	F	H	L	N	P	R

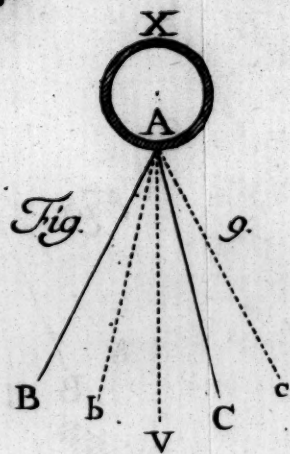


Fig. 9.

Fig. 14.

I A M

Fig. 11.

D	H	N	R				
A	C	F	G	I	M	O	Q
B	E	L	P				

Fig. 16.

D C B A M
E F G



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I

F

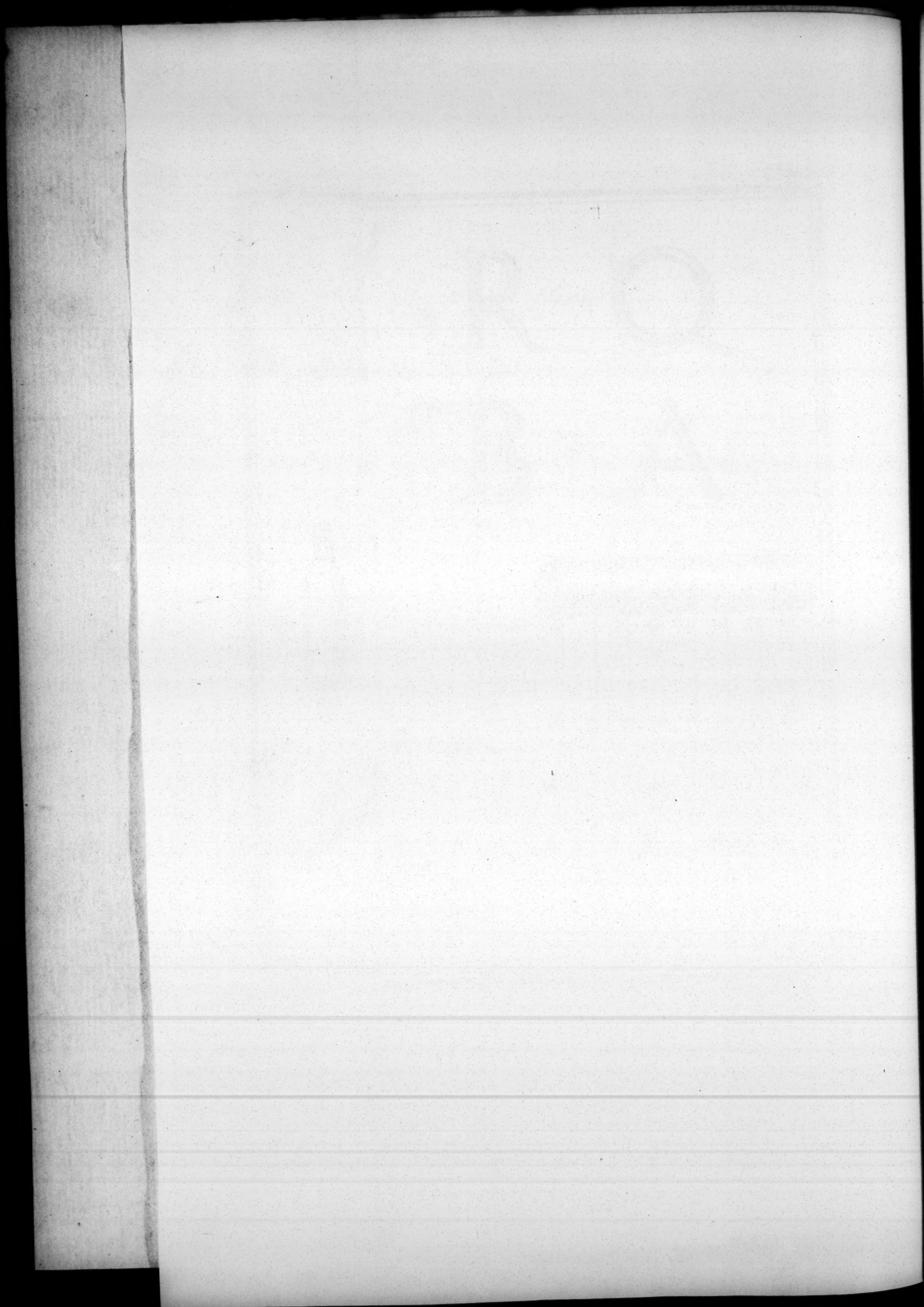
G

H

M

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S E C T I O N III.

On vivid Electricity ; otherwise, on electric Sparks.

C H A P. I.

On sparks relatively to the air.

519. **W**E have hitherto considered the modification of the pressure that takes place in the electric fire proper to the air around bodies, in consequence of the electric fire that is added to, or taken from the surface of those bodies ; now we are to consider the action of the air against the fire, exuberant relatively to it, that passes through it under the shape of a *spark*, and also that of the spark against this air. In regard to these reciprocal actions, two things must be attended to ; the one is, the kind of proportion according to which the air resists the spark ; the other is, the peculiar kind of action which electric fire exercises on air. To that end, we must chiefly observe three accidents ; I. The *extenuated* shape of the sparks. II. Their crooked direction. III. Their division.

SECT. I. *On the resistance which air opposes to sparks.*

520. *Air resists sparks in proportion to its own density ; in proportion to the thickness of the stratum it opposes to them ; and in proportion to the length of the passage which sparks are led to open to themselves through its substance.* In order to conclude *universally*, that air resists the electric fire, it is enough to observe, that it forms, jointly with other insulating bodies, the general *inclosure* which preserves on the surface of bodies their electricity. In regard to the *particular* conclusion above expressed, viz. that air resists the electric fire, according to the above mentioned proportions, the experiment I formerly described in num. 114 of *Art. Elec.* will be of great service to prove it. With wax made soft with turpentine, I fix a re-

ceiver on the basin of the air-pump, with a brass rod adapted to it, in such a manner that this rod may be raised or lowered within it at pleasure (Pl. IV. fig. 12, 13, 14, 15.) I make this rod communicate with the Chain, and before I draw off the air, I take notice, that, from the extremity B of the rod (fig. 12.) which remains at six inches distance from the basin, the brush springs downwards from the rod, in the usual manner, to the distance of about an inch. This done, I make the air to be successively drawn from the glass-bell; and, at every stroke of the embolus, I observe that gradual changes successively take place in the brush, and that in proportion as the air is farther extracted: I. The rays of the brush are lengthened. II. Their mutual divergence is lessened, III. as well as their number, IV. and their sparkling. V. The width of the remaining rays increases, VI. as well as their continuation; so that, when the mercury of the barometer within the glass-bell has been brought to be no more than an inch high, there remains only four or five rays, which, from the rod, reach quite down to the basin; and if the mercury be still more lowered, till it is brought to be only four lines high, there remains nothing under the bell (the brass rod must then be a little lowered, as in the fig. 14.) but a continued red-violaceous ray. To all these observations the following fact is to be added, viz. that in proportion as the air grows rarified, at least beyond a certain measure, the electric signs lessen in the Chain; so that when the air is brought to its greatest rarity (this I observed before with respect to the experiment of Dr. Hawksbee), sparks intirely cease, and the electroscope diverges no longer.

521. If the rod AB is left at a great distance from the basin, and especially if it be very sharp, then, if the air be extremely dilated, the single continued ray ceases to appear, but a short light appears on the said point, like the light mentioned in numb. 139, 141. and has been called light of *diffusion*, which extends to a certain distance in the direction of the sides of the point, and grows gradually fainter, so that at the distance of an inch it is perceived no more. But when the experiment is made in the dark, a very weak light is discerned, which extends from the point, and forms

forms an ample, though very rare, brush, like that represented in fig. 15; and a very good eye may, moreover, if the darkness be complete, perceive a kind of spurious brush (119) more languid and short, which reaches the knob C on the basin of the air-pump. When the rod is thus placed at a great distance from the basin of the pump, whatever may be the rarity of the air, a faint motion continues to be discernible in the electroscope annexed to the Chain; this is, because the air, notwithstanding its great rarity, yet resists in proportion to the great height or thickness of its stratum (the rod is here supposed to be ten inches, or a foot distant from the basin); it, therefore, keeps back a part of the electric fire, which would, if the stratum of air was less, intirely and freely pass through it. For the like reason also, the excessive fire which through a short stratum of rarefied air, readily flows, united into a continued ray, divides and spreads itself, when the same stratum is lengthened into those long and almost invisible flashes.

522. But, in order more accurately to distinguish the changes with respect to its form and continuation, which the electric fire suffers in air which is gradually more and more dilated, the experiment must be made with rods armed at their ends with balls (Pl. IV. fig. 6.). Let such rods be brought to the distance of one inch from each other; then, when the vacuum is rendered as exact as possible, a single, continued, uniform ray, of a red-violaceous colour, is seen in the dark to flow from the rod B to the rod C. In proportion as air is afterwards admitted, this ray acquires, first a kind of quivering motion, which indicates the beginning of an interruption; then, a real interruption with an undulation and division of the ray, take place (especially if the balls be a little removed from each other); afterwards the ray acquires a somewhat more vivid light, so that, through a continued succession of changes, this quiet, continuous ray, at last turns into very vivacious sparks, discernible even by day-light, though, it is necessary to observe, they are emitted only with such interruptions as are necessary to make the fire rise in the Chain to a sufficient density, to enable it to surmount at once the resistance opposed by the external air.

523. If

523. If the balls B, C, are kept at a still greater distance from each other, then the ray that joins both begins to appear less uniform, though the air be dilated to the utmost, and an interruption of the fire takes place in the middle between the two balls, which increases as they are removed farther from each other. About twenty years ago I had a thought of amusing myself with a kind of electric lucid writing; I procured glass tubes bent into the shape of letters, like those represented in the Pl. IX. fig. 4, 5; iron wires were hermetically inserted into their extremities, and after the air had been accurately drawn out of them, they were hermetically closed. But I had scarcely begun to experiment, when I perceived that a defective reasoning had led me into a vain attempt: so far was I from illuminating the whole series of letters (which communicated together through the iron wires), that I could not completely illuminate even a single one of them, with the whole discharge of a bottle; the reason was, because the space was too great to allow the spark, however vehement, to go through the dilated air contained in the above tubes.

524. In the barometric vacuum, as it is more complete, the light of a spark reaches to a much greater distance. While I stand insulated (as I said in pag. 48 of the *Terres Atmosf. Elect.*) and communicate with the Chain, I touch the hollow ball into which the upper part of my barometer is terminated, and a little spark leaps between my finger and the said ball; meanwhile, an electric light, of a purpureous colour, fills the whole empty space in the barometer, so far as the surface of the mercury; but, on the other hand, it is to be observed, that this light is more dilated, and affects less the eye, in proportion as it surpasses in point of extent the spark that has been thrown from my finger to the ball.

525. These experiments may be varied at pleasure, and even are rendered more significative by causing the sparks to pass intirely through a barometric vacuum; which may be effected with the double barometer ABCDE (Pl. VIII. fig. 10.) contrived by the ingenious philosopher, Lord Cavendish. I have several barometers of this construction, in which the empty space BCD extends to a distance somewhat greater than a foot. From the bottom of the two small canns A, E, into which the two arms of the

the barometer are inserted, two iron wires rise, terminated into balls; I grasp the one of these, and present the other to the Chain; then, I. The whole empty portion BCD appears uniformly illuminated with a light somewhat stronger than that which takes place under the glass bell, between the two brass balls. II. When I send whole discharges of bottles through the said space, the light appears uniform throughout the same; but it is more white and more splendid, in proportion as the discharges are stronger; that is, as the quantity of fire that passes through the vacuum is more copious. III. To this add, that this light lasts only an instant; and the shocks those discharges give me, are both strong, and like those which I send immediately through my body.

526. These different accidents afford an explanation for the second case above mentioned; that is, they explain the changes of colour which electric lights exhibit in certain circumstances. These changes depend on the greater, or less, rarity of the electric fire: according as it flows with more density, it throws a light whiter and more intense; in proportion as it is more dilated, it emits a light weaker and more violaceous; and these different appearances equally take place in the barometric vacuum, in air that is only somewhat dilated, and in the atmospheric air. This same principle explains also the various appearances exhibited by the electric fire in natural phenomena; how it dazzles in a thunderbolt; how it does not offend the eye in an extensively diffused flash of lightning; how it even creates a pleasing sensation, when exhibiting itself under the shape of a falling star; and how it assumes various reddish appearances in the *aurora borealis*; but of this I shall treat more largely hereafter, and shall only observe here that a few other circumstances besides those above, concur in producing those variations of electric lights.

527. All the said accidents moreover prove, that *a complete vacuum may be considered as a medium pretty exactly deferent; and that this deferency gradually lessens in proportion as the exactness of the vacuum diminishes.* In fact, if I successively introduce air-bubbles into a double barometer, the empty space BCD ceases to be intirely illuminated, but the light gradually gathers itself nearer to B and D; and in C, it either becomes very languid, or
intirely

intirely ceases, according as more bubbles of air have been introduced; correspondently also, the lights in B and D appear more vivid, and last longer; the discharge is effected with a less stroke, and after it, a greater portion of the charge is found to have remained within the bottle.

528. I shall have occasion to mention again the reason why the light of the fire that passes through an incomplete vacuum, is interrupted in the middle between two deferent bodies placed in it; here I have sufficiently demonstrated, I think, that air opposes sparks in proportion to its own density, and in proportion to the thickness of the stratum through which the latter are led to pass. The resistance of air is moreover proportioned to the *width* of the passage, which the spark opens to itself through it, and a very simple experiment may prove it. If to the head M (Pl. II. fig. 8.) of the conducting bow MNO, balls of gradually larger diameters are successively adapted, the spark that will fly to such balls, will be larger, will leave a broader mark on the balls, and produce a sound of a graver tone. But then it will only leap to a less distance; for the same strength which would be employed by the spark in driving a longer column of air, were this spark drawn by a less part of a lesser ball, is now employed in expelling a shorter, it is true, but a *wider* column of air, in consequence of the spark being drawn, as we suppose, by a larger part of a larger ball.

529. If the bodies between which sparks are excited, remain constantly of the same size, then the above proportion between the resistance of the air, and the width of the passage which the spark is made to open to itself through it, holds still; but here we must observe, that *the width of the passage will rather correspond with the quantity of the fire that is thrown, and the length of the same passage will rather correspond with the degree of density to which the fire is brought.* This is the reason why a simple spark from a conductor leaps to a distance, at least equal to that to which a middling discharge of a bottle will do; for, though the quantity of the fire thrown from the bottle surpasses ever so much the quantity of the fire that leaps from the conductor, yet the density in the former is not superior to the density on the latter; which is evident from this bare consideration, that the density of the

the fire in the bottle only proceeds from the density of the fire on the conductor.

530. With regard to the proportion that obtains between the breadth of the spark, and the quantity of fire, Dr. Priestley has made very elegant observations. That gentleman trying with a harpsicord the tones of the sounds produced by the explosions of various sparks, found (p. 717.) that their gravity corresponded with the extent of the coatings from which the sparks were excited; that is, with the quantity of the fire. These experiments might be carried farther, by placing small bits of paper astride on the strings of a harpsichord, and attentively observing which of these would be made to leap highest by the different explosions of sparks excited from jars of various capacities.

531. Upon the whole, I think that in extensive streams of electric fire, the copiousness of it, while it tends to increase the spark, greatly contributes also to spread it. I also think, that the *deferent* vapours which lightning excites, and as it were supplies itself with, are like a vehicle that greatly serves to increase the *ampleness* of its passage; but the copiousness itself of the fire, which at first partly is thrown at once, and partly succeeds afterwards, must undoubtedly also contribute to produce the same effect. From the beginning I observed, that the effects of sparks increase, in proportion as the sections of the medium through which they pass, lessen; and that this increase of efficiency ought to be imputed to the greater density which the fire possesses in those narrower sections. It is true, that, besides a condensation of the electric fire, a retardation of the same must also take place in such narrow sections; but if the condensed stream of fire be followed by new successive *waves* of it, as we may conclude it, to be the case, from the uninterrupted blaze and continual successive explosions that take place in lightning, it is evident, that the new succeeding flashes must contribute to promote the motion of the whole stream.

SECT. II. *On the manner of the action which electric sparks exert on the air.*

532. I think I have been the first who thought of investigating the nature of the action which the electric fire exerts on the air

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through

through which it passes. This I have done, first in the fifth chapter of *Artif. Elect.* where, from the *silence* of the electric fire, when it is merely extracted from, or added to, bodies, and from the noise produced by it, when it rushes from a point into the air, I concluded that sparks vibrate the air through which they pass; and I was thence induced to try, by the means of a tube of glass fitted for that purpose, what kind of impression the air agitated by sparks would make on the liquor contained in that tube.

533. The following is the manner after which I usually make this experiment. I. With some cement, or also by means of a lamp, I seal the end A (Pl. V. fig. 15.) of the tube AB, around the iron wire DR. II. I warm the tube a little, and dip the open orifice B into a glass of wine, so that it sucks it in, to the height of one or two inches. III. Into this open orifice B, I insert another iron wire BU, whose end U remains distant from the end R of the other iron wire RA, four, eight, or more lines. IV. And in the instant when I send the discharge of a coated sheet of glass through the two wires, the air, struck by the spark in the whole interval RU, transmits the stroke in C against the surface of the wine, the whole, or part, of which, is driven out at the open orifice B.

534. In repeating this experiment in tubes of various lengths, and with more powerful sparks, I observed, that the part of the small cylinder of wine (I use red wine in order the better to discern the various accidents that take place) that had been pushed towards the orifice B, but had not been driven out of it, recovered its former station in S, with a perceivable degree of slowness. Thence I concluded, that the spark, in passing through the space RU, introduced a kind of permanent expansion in the air in it; which did not give way but gradually, and in the same manner as an expansion introduced by heat might have done: I have since been confirmed in this opinion, by observing, that the length of the cylinder of wine which is driven out, greatly exceeds, when the spark is a vehement one, the interval RV, that is the space through which this spark passes.

535. In *Atmos. Terr. Elect.* desiring to ascertain whether the air in the above mentioned experiments was driven in every direction

tion, I made a hole with a file in the middle of a tube; then with sealing-wax I fitted another tube to it, perpendicular to the place where the hole was made, and both thus communicated together through this hole. I made afterwards the perpendicular tube suck wine in, to the height of eleven lines; lastly, I sealed with sealing-wax an iron wire to each end of the transverse tube (the two tubes formed together an appearance like that of a T); which extended to the hole of communication. The spark being afterwards sent through the wires; a part of the wine was driven out of the perpendicular tube; and thus I ascertained that air is driven by sparks, with a direction even perpendicular to the direction of those sparks themselves.

536. I have also made several other experiments of the same kind with the former. At p. 78, I mentioned one in which I made the air drive wine into different and contrary directions, from both ends of a tube. In the fig. 16, of Pl. V. a tube AB is represented, which is hermetically sealed at both its ends A and B, around two iron wires, which are brought near each other in CD; perpendicular to the interval left between them, a kind of spout of glass F is annexed, through which, at the same instant that the spark is thrown, the air blows, and drives a little heap of dust placed before the spout, or sets in motion a small moveable wheel, and also puts out a small light.

537. In fig. 14. four tubes are represented annexed to each other, in the shape of a cross. I make each of them suck wine in; into two of them I insert two iron wires, and the spark, in the moment that it leaps, drives part of the wine from each of the tubes.

538. In the year 1764, I described in a small book of observations which I sent to the duke of York, the apparatus which is represented in fig. 11. Pl. III. I. The brass rod Dc, which rises from a support D, is inserted through the bottom of a bottle, and is sealed to it. II. The rod Ab, which in A is terminated into a ball, is sealed within the neck of the same bottle, and the two rods are placed at a convenient distance from each other. III. To the neck of the bottle, the small tube, or syphon *efg* is annexed, which I make suck wine in; and when the spark rushes through

the interval *bc*, it lowers the wine in the part *ef* of the syphon, and raises it in the other part of it *fg*. It was only in the year 1770, that I saw in Dr. Franklin's work, that Mr. Kinnersley had wrote to him from Philadelphia, a letter in which he describes a similar instrument with the above, which he calls an *aereo-electric* thermometer: his letter is dated in the year 1761; that is, is nine years posterior to the first experiments I made on the same subject. Likewise, before I had any knowledge of the above instrument, I wrote to the Royal Society of London the description of an experiment in which I raised the mercurial column of a barometer by means of electric sparks. CBG (Pl. V. fig. 17.) is the tube of that barometer; it is divided in G into two tubes, and the mouth of each of these is sealed, in D and in E, around two iron wires, which remain parted from each other in S; the tube AB is fastened to the board AB, which rises from the support H T R. This support is placed near an electric battery, and it communicates, through the wire EF, with the linings of the jars that form that battery; then, touching the coatings with the one end of the conducting bow, and bringing the other end to the wire D, I find that the spark which is thrown from S, lowers the surface of the mercurial cylinder in B, for one or two lines, or even more, according to the quantity of the charge.

539. In short, in whatever manner the action of sparks on air, be investigated, every accident that happens concurs to prove, I. *That air is driven proportionally to the copiousness and density of the electric fire.* II. *That it is driven in every direction.* III. *That it is driven by a kind of dilating force which does not instantly cease, but is somewhat continued, and is like to the dilatation that takes place in it, when a new degree of heat is introduced into it.*

SECT. III. *On the extenuated shape of sparks.*

540. I have not as yet read that any body has observed a particular kind of appearance, which pretty constantly takes place in the shape of the electric sparks that are drawn from a conductor: these sparks appear more splendid, more vivid, of a whiter and intenser

tenser light near their extremities, than in their middle, where they appear of a red violaceous colour, more languid and narrow; and the place where the light thus grows languid and narrower, is usually most distant from that of the two bodies between which the spark is thrown, which is most convex, or sharp, and nearest that body, which is least convex, or flattest. Thus, when I bring the knob of the brass cylinder (516) near the conductor, that part of the *extenuated* spark which I mention, remains the nearer the conductor, in proportion as the latter is less convex than that knob.

541. This phenomenon has appeared to me worth considering attentively, either on its own account, or because it may seem at first sight to favour the doctrine of the *affluency* of two distinct electric fluids, which lighten when separated, and vanish from sight when they join together. Being at a loss how to resolve this difficulty, I have undertaken to examine the passage which sparks open to themselves through a thick quire of paper, and have found that this *passage* also appears *extenuated* in its middle, and exhibits some likeness with the shape of the sparks which pass through the resisting medium, air.

542. Discharging, therefore, at several times, a coated plate of glass, through a mass of six sheets of paper sewed together, I constantly observed, I. That the edges of the holes made in the upper leaves by the sparks, were bent upwards; and in the lower leaves the edges of the holes were bent downwards. II. That in those *upper* leaves which were at top, and lay near the conducting bow, the edges of the holes rent in them, were both larger and more bent upwards, than in the leaves next under them; and correspondently, the holes were broadest, and the edges of them most bent downwards, in those *inferior* leaves which lay nearest to the coating of the plate. III. In those leaves which lay about the middle, the holes were so narrow as to be hardly discernible, and the extremely small bur which was around them, seemed, in the leaves on the one side, to be raised upwards, and in the other leaves to be thrown downwards.

543. But we must observe, that in order to obtain this gradation, both in the size of the holes, and the extent of the bur around them, the quire of paper must be used just as it is taken from

from the ream without opening its leaves, and must be placed softly on the coated plate, without pressing upon it, otherwise the above gradation in the successive holes will not obtain. If the quire remains with its lowest leaf closely united to the coating of the plate, the width of the hole, and the bur around it, will both be less; and if the leaves in the middle remain at any distance from each other, the holes made in them will be larger, and the bur be directed towards that part where the separation takes place: these observations have suggested to me the following experiment. I. I part the leaves of the quire into two distinct series; that is, I place a fold of eight leaves between the sixth leaf and the seventh. II. At another time, I part the leaves into three series, putting the same distance between them as I did between the two former. III. Again, I part them into four series; and, in all these cases, the holes made by the sparks keep the following rules. I. The widening of the holes, and the direction of the bur around them, always proceed from the middle of the leaves that are united together, towards the places of separation, with contrary directions. II. Meanwhile the differences between the sizes of the holes grow less, as does also the bur around the holes, in proportion as the number of the partitions between the leaves has been increased.

544. From the constancy of those facts, it results first, that the reasoning of those who conclude from the burs around the holes being turned outwards in contrary directions, that there exist two distinct contrary streams of electric fluids, is ill grounded. First, according to that hypothesis, such streams cannot be supposed to meet each other within the leaves, but must rush from the middle of them towards opposite parts, conformably to the direction of the bur; and, secondly, our last experiment makes it also necessary to suppose as many *pairs* of streams of the electric fluid, as there are partitions made in the quire of paper, which is an absurdity. If it were insisted upon, that the streams of electric fire, instead of rushing from the middle of the leaves, meet there, the same absurdity from their multiplicity still exists; and, besides, there would be a downright contradiction to facts, because in such case the bur would be bent inwards, towards the middle of the leaves, where such streams, as it is supposed, meet.

545. On

545. On the contrary, the following is a very simple, as well as a necessary principle, viz. *that the electric fire tends to explode, in every direction, the parts of the resisting substances through which it passes.* I availed myself of that principle (*Art. Elect.* p. 136.) to explain the common experiment in which the bur around the hole rent by a spark in a piece of pasteboard is always thrown outwards on both sides; now, I shall proceed to describe the several accidents that take place in my new experiment of the *partitioned quire*; only, I shall add to the above principle, what indeed is a necessary consequence of it, viz. *that the actual explosion of resisting substances into various directions, is in an inverse ratio to the resistance, or force, that supports those substances in those directions.* If we meant to express this principle more clearly, we ought to say, *that the electric fire displaces the parts of the bodies it meets with, not by striking against their surfaces, as ivory or chalk balls do in experiments on the collision of bodies, but by traversing, that is, by penetrating, through the pores of such bodies, and by exerting in its passage its expansive force, condensating and driving forwards what fire it finds inherent within those bodies, and experiencing, at the same time, a re-action from them equal to its own action.*

546. These principles being now laid down, we may easily conceive, I. That the spark, at the instant that it forces its way into the internal pores of the whole united quire, must, in consequence of the equal resistances opposed to it, both by the six upper leaves, and by the six inferior ones, must, I say, burst the former upwards, and the latter downwards, since the thick substance of the paper around the holes does not allow it to produce any lateral effect. II. The inside leaves will resist more than the outward ones, because their burs, when driven either upwards or downwards, must needs get into the holes of the next leaves; and consequently cannot attain the given width of those holes, but by distending and opening them still more. III. Whence it follows, that in the inside leaves, the spark must grow continually narrower, and open narrower holes, since the resistance it meets with continually increases.

547. From these observations it follows, that in every series of leaves joined together, whether there be one, two, or three such.

such series, the same phenomena must result in each of them severally, that take place in the whole compact quire; with this additional circumstance, however, viz. that according as the leaves will be fewer, the difference between the internal and the external holes will be less, since the external holes will, in such case, be distended by fewer successive burs; and reciprocally, the internal holes having fewer external holes within which they must dispose and lodge their own bur, will the more readily open and widen.

548. Meanwhile, since the explosion in contrary directions, which in the *united* quire is single, becomes multiplied in the *partitioned* quire, in consequence of the intervening of sufficiently thick strata of air, it plainly appears, that it is the united effect of the successive resistances which take place, that makes the spark act back against the former half part of the same.

549. I take another compact quire from the ream, I sew it in the same manner as above, and between the sixth and the seventh leaf, I now insert a sheet of tin, like those used for looking-glasses; and I find, that the bare interposition of this thin deferent substance, is sufficient to cause the bur around the holes, in each of the two series of leaves, to be directed into two different and opposite ways; the three first leaves of the *upper* series have their bur directed upwards, and the three others have it directed downwards; the same takes place in the *lower* series, only the spark usually divides itself within it, into two or more sparks. It is, however, necessary to observe, that the holes that took place near the intervals filled by air, in the former experiment, were smaller than those which were rent near the head of the conducting bow, or near the coating of the plate; but the holes which are rent close to the tin sheet, in this experiment, are equal to the external ones, with respect both to their width, and the extent of their bur; this is because the spark can more freely expand itself through the deferent metal, than through the intervals filled by the moveable indeed, but *cohibent* air, and the tin sheet better interrupts the continuation of the resistances from the successive leaves of paper, than a stratum of air can do.

550. In the above experiment with a metallic sheet, we may observe how, to the particular action of the sparks against the quire

quire of paper, corresponds a particular reaction of the resisting quire against the sparks, in consequence of which, the latter remain particularly narrow and condensed; of this a proof may be seen in the above tin sheet itself, which is more completely melted by the spark, when it is traversed by it within the quire of paper, than when it is traversed in the open air.

551. But to return to our principal point; when I first examined the kind of contraction that takes place in sparks when passing through air, I thought it was to be explained in the same manner as the increase of contraction that takes place in them, when sent through quires of paper; but considering afterwards the subject more attentively, I could not be satisfied, and proposed to myself the following questions: I. Why should not air yield equally throughout its whole substance? surely the constituent particles of it are exceedingly moveable, and entirely free from any such adherence to each other, as obtains between those of paper. II. Ought not sparks, when contracted in air, to acquire a greater density, and of course a more vivid light, in the place where such contraction obtains, than in any other; in the same manner as it happens within the quire of paper, where the sparks, by condensing themselves, more completely melt the sheet of tin than in the open air? III. Does such contraction proceed from the superior rapidity with which sparks rush, when in the middle of their passage; in the same manner as rivers grow narrower when their velocity increases? and is such rapidity apt to prevent the fire from making any strong impression on our eyes?

552. This I can certainly affirm, *that the electric ray that passes through air extremely dilated, does not suffer any contraction like that which sparks suffer when they pass through atmospheric air; nor does that ray suffer any weakening of its light in its middle.* The ray which in a complete vacuum passes from the ball B to the ball C (Pl. II. fig. 9.) an inch distant from the former, is perfectly continuous, and equable in its whole length: its light indeed is much inferior to that of sparks, but then it is uniform throughout; therefore, it is evident, that the contracting and weakening of sparks in their middle, whether apparent, or real, must be attributed to

some particular action exerted on them by the air through which they pass.

553. And it is in that view, that examining more exactly the light, which I have called of *overflow* (142.) I found that it is not to be imputed solely to the reaction of the electric fire in the ball C, into which the electric ray BC rushes, as I have supposed in num. 146; but this light (which is represented in the fig. 10, 11, and 12, pl. V.) is proportioned to the remaining quantity of air within the receiver, as well as to the quantity of the fire which passes through it; so far, I mean, as it results from the reaction of this same remnant of air, independent of the reaction of the electric fire in the ball, into which this fire passes.

554. I think, however, that two considerations of the same kind, as those with which I explained the light of *diffusion*, and that of *overflow* in dilated air, will also serve to explain the present difficulty, that is, why sparks in atmospheric air, are larger at both their extremities than in their middle. If a small quantity of fire issuing from the surface of a ball, is retarded, and in some degree kept back to it by air much dilated, so that it shines around the ball from which it proceeds, why should not atmospheric air, which is much denser, also repress and keep back in some degree the copious fire of sparks, when this fire rushes against it? This seems at least to be a necessary consequence of the same power in air, which causes the accumulation of electricity around different bodies, previous to its leaping from them, under the shape of sparks (553); and the minute sparks which constantly precede the darting of the main sparks, confirm this conjecture.

SECT. IV. *On the crooked direction of sparks across air.*

555. I flatter myself that I have, in num. 516, 517. sufficiently explained the cause, and laid down the law, of the *continued incurvation* of sparks, which from the surface of one body run to another body presented to it, in a situation almost parallel. This cause may be expressed in universal terms; *whenever a spark,*
being

being determined by the direction of its stream to move in a right line, meets with a resistance from air, it deviates from its former course towards some other place where it finds less resistance. This is the same principle of which I made use in my first Work on Electricity, in order to explain the mutual divergence of the rays that form electric brushes. Present, I said, your finger laterally and somewhat underneath, to the electric brush which springs from a very sharp point, and you will see it bending and lengthening itself towards the tip of your finger, with a continued incurvation; the reason is, that as the stratum of air through which the rays of the brush endeavour to pass, is lessened by the approach of your finger, which prevents their deviation from each other, as well as their deperdition, and causes them to stretch and gather towards your finger.

556. And would not this principle, that air resists more in proportion as the thickness of its strata increases, besides the common curvature of sparks, also explain the complicated motions of certain sparks, which appear as if they were broken, and generated by repeated reflections? The French electricians call them *zig-zag* sparks; we might as justly call them *capricious* sparks, since they change so suddenly their directions, that they appear to us, who are ignorant of the cause, as if prompted by kinds of whims. We might also call them *hooked* sparks, since their successive flashes are often inclined to each other, at very acute angles, like hooks. In order to investigate whether there is any regularity in these motions, seemingly so irregular, I shall lay before the reader several observations which I have made on this subject.

557. First, I have observed, that with a series of deferent bodies properly disposed, I may easily render the direction of a spark as crooked as I please: it was in this manner that I represented, in num. 539 of *Natur. Elect.* the *zig-zag* direction of lightning; that is, I made a spark pass along a series of small metallic balls placed near one another in a crooked line, on a cube of wax. At other times, proceeding on the same principle, I made the spark pass in the middle through a series of corners of metallic sheets, so disposed as to make the spark represent in its way letters, branches of trees, &c. But now, though it be a fact, that

sparks will rush to such deferent bodies as lie in, or near, their way, yet the supposition of a fortuitous disposition of deferent bodies, does not completely account for the particular mode of the direction of those sparks which we have called *zig-zag* sparks; the above mentioned motions of sparks towards deferent bodies, constantly appear to be only continued progressions of the same towards a given point, through the least resisting medium; but their *zig-zag* motions seem as if they were the effects of resistances that actually reperiuss those sparks, and throw them back.

558. Secondly, I observe, that it seems that the intervention of deferent bodies, rather hinders than favours the *zig-zag* direction of sparks. When I present a deferent body laterally to the passage of a spark, the latter *zig-zags* less than it otherwise would do. Lightning, when it rushes through the body of a cloud, has, at most, a slight flexure in its direction, and no *zig-zag* takes place in its motion; the deferent cloud affording to it a passage continued enough for it to run in a strait line, or nearly. Flashes which rush through bodies of clouds, exhibit the appearance of long rectangular bands; and they seem to *zig-zag* in those places only where they rush through tracts of air that are free from any great quantity of deferent vapours. I often discharge the coated plate AB (Pl. VI. fig. 9.) through a drop of mercury, which I place on the naked margin HC; and I find afterwards that the passage of the spark is marked by the effluvia, or fumes of the mercury, which the spark has left imprinted on the plate; this track indeed is somewhat tortuous, but less so than that of such sparks as pass directly through air, without the assistance of any vehicle.

559. As a confirmation of this principle, that sparks and lightning *zig-zag* most when they pass through air free from vapours, I observe, in the third place, that either in the air-pump, after the air has been accurately drawn off, or within the pretty large balls of my barometer, sparks never have the least *zig-zag* appearance: it is true, I spoke, in num. 283, of flashes of fire which moved with a flexuous motion within a glass-bell emptied of air; but I observed, at the same time, that these flashes flowed close to the inner surface of the bell (284), and that this was owing to the
tortuous

tortuous disposition of some parts of that surface, which being less *resisting* than the rest, invited the fire thus to wind close to them. I even can, at pleasure, produce such a flexuous motion of the electric fire as I mention here; I raise the rod A B (Pl. IV. fig. 12.), so that it remains at a considerable distance from the basin; then, with my wetted finger, I describe a crooked line on the glass-bell, which, from the brass armature of its neck, reaches down to the basin; and I immediately see that the electric fire begins to wind about, close to the inner surface of the bell, correspondently to my finger; this fire being now enabled to adhere and accumulate itself on this inner surface, because it now can, through the deferent dampness affixed to the outside of the bell, drive some fire from that outside. But when the ray within the bell is, as it were, left to itself, and the air is accurately drawn from the bell, then this ray runs between the two balls B, C, (Pl. II. fig. 9.) in a direct line. If the two balls be removed to a greater distance from each other (Pl. IV. fig. 6.), and the remnant of air be somewhat considerable, then the ray will indeed fail in its middle, and divide at its extremity, but its zig-zag motion will only begin to take place when a very considerable quantity of air has been let into the receiver. Now, do not these facts render it manifest, *that the zig-zag directions of sparks chiefly arise from the reaction of the air, against them?*

563. But how can we ascertain the precise degree of this reaction, which is necessary to begin to produce the zig-zag motion of sparks, since their continuous incurvated direction has been proved to be also the effect of this reaction? With respect to that, I first observe, that in electric brushes, there is not the least appearance of a zig-zag motion; now, how does it come to pass that the sparks which form those brushes, thus rush through a long tract of air, and yet are never thrown back, but only continuously incurvated?

561. To this observation we may add this other, which is an exception to the former, viz. that sparks which move with incurvated directions, are not, however, always without some appearance of a zig-zag motion. Those sparks which, from the conductor, leap to the knuckle of my finger, or to the head of a cylinder

cylinder presented to the conductor, in a situation almost parallel to it, always exhibit some zig-zag motion mingled, as it were, with their *incurvated* progression; as for brushes, they constantly bend themselves towards my finger, in a regular manner, and without any tortuosity, only the nearest rays bend themselves most; those which are farthest, least of all.

562. *It appears that it is the rapidity of the blow struck by sparks against the air, which causes the latter to stop their course, turn them, and reperfuss them; in the same manner as the rapidity of the blow of a stick against water, causes it to jump back; or, in the same manner as water put in a pistol, and shot out of it, is reperfussed back by the air it strikes against.*

563. In fact, I observe in the sixth place, that such sparks as are thrown with a greater force than others, and are consequently enabled to rush through longer tracts of air, also exhibit more complicated zig-zags, and deviate from their right course through longer spaces and with longer flashes, than others do; this is rendered evident, by observing the different directions of lightning, and even of electric sparks when their intensity is increased.

564. It appears besides, that air, in consequence of its moveableness, is peculiarly apt to reperfuss sparks, and turn them out of their right course through *hooked* directions. My reason for thinking so, is, that sparks sent through mediums more compact, but then of another nature than air, assume no such hooked motions. Once, in very favourable weather, I sent the discharge of my three large crystal jars through six quires of paper stitched together; and, examining them afterwards, I found that the long hole made through them was in a strait line, that is to say, *the great lateral resistance of the paper had kept the spark in its strait course; whereas, the moveableness of the particles of air, allows sparks to run through different tracks of the same, when reperfussed in consequence of the violence of their own motion.*

565. Lastly, it has appeared to me, that such sparks as obliquely rush against air, exhibit a still more complicated hooked direction than others do; the reason of which, I think, is, that as the stream of fire strikes unequally against air, it is more easily
turned

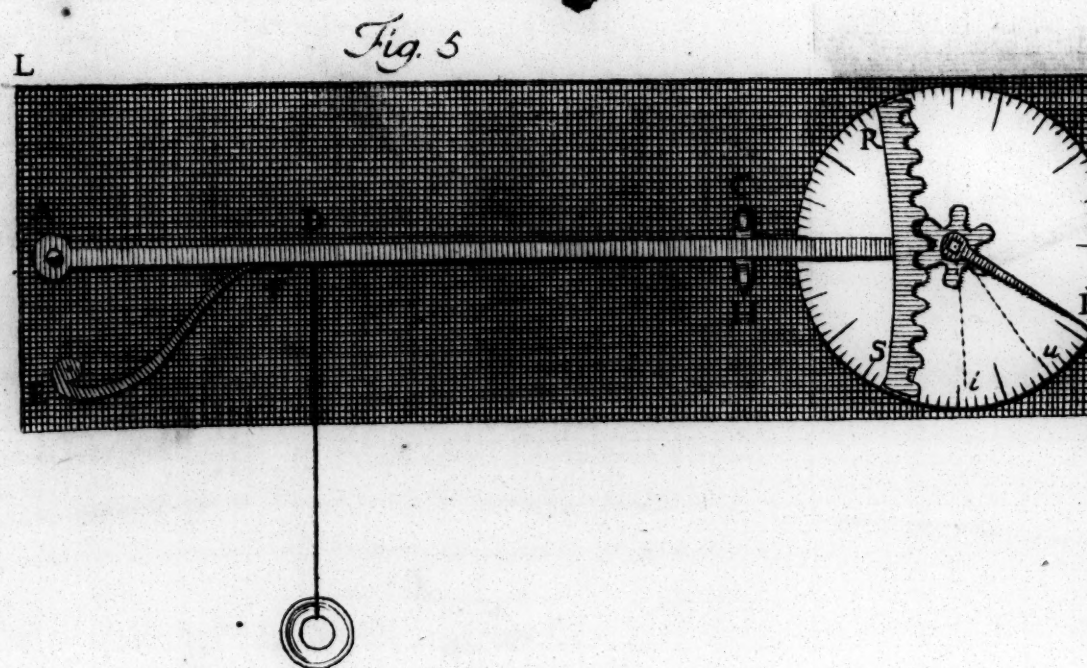
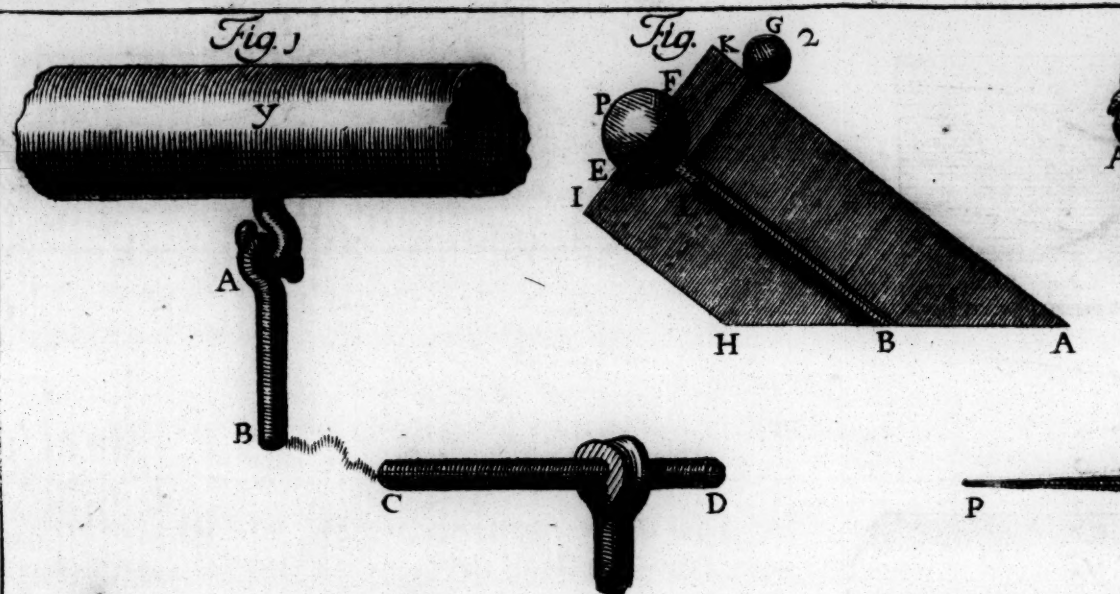


Fig. 3



Fig. 4



Z

Fig. 5



Fig. 8

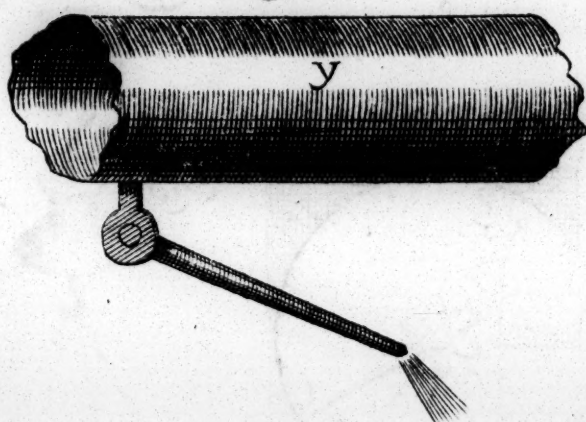


Fig. 9

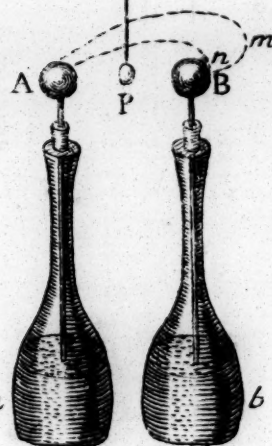


Fig. 6

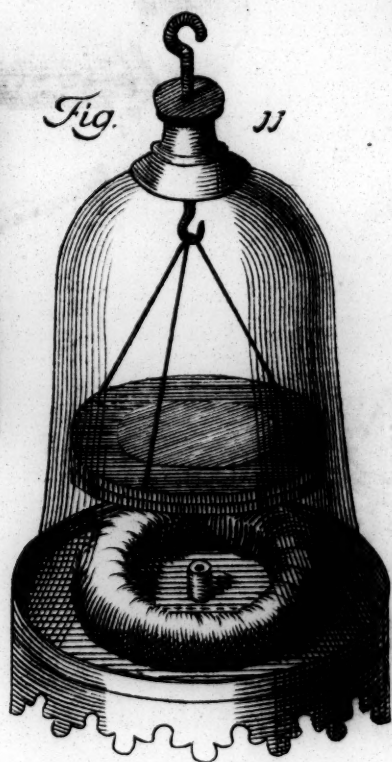
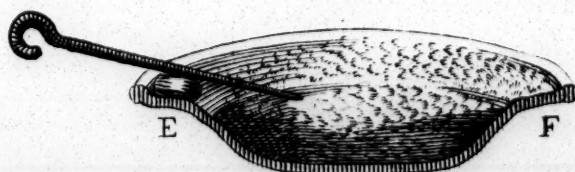


Fig. 7





turned out of its right course towards that place where it strikes with the least force, than towards any other. The truth of this observation I commonly investigate by means of the following experiment : I. I adapt to the conductor Y (Pl. X. fig. 1.) a brass rod AB, two lines thick, and rounded at its extremity. II. On the support of glass U, I fasten horizontally another similar rod CD, which is likewise rounded in C; I dispose this second rod in such a manner, that it be some few lines lower than the end B of the rod AB, and its end about sixteen lines distant from it. III. Then, as often as the conductor Y has again received a charge, I present my finger to the end D of the rod CD; when sparks take place in BC, which are the more crooked, as their passage is rendered longer. These sparks are usually thrown from B with a kind of a downward direction, but they immediately bend their course towards the other rod, and after this first curvature, which is rather of a continuous kind, their direction becomes remarkably crooked.

566. I shall here propose a question; why does the spark after being repercussed towards B, yet continue to move towards C? This, no doubt happens, because in AB there is always a remnant of excess, which preserves a degree of tension in the fire of the ambient air, and the consequence of this tension is, that the spark is again repelled; in fact, we shall soon see, that electric brushes, which may with truth be considered as aggregations of infinitely small sparks, are repercussed and made to deviate, when they meet with *redundant* atmospheres.

567. To which add this other consideration, viz. that the redundant atmosphere around the rod AB, and much more so, the efficiency of the spark itself when it leaps, always drive some natural fire both from the surface of the rod CD, and from that of the air contiguous to it; so that the tension of the natural fire in the air immediately contiguous to the rod CD, becomes lessened, and this air thus opposes less resistance than any other place does, to the passing spark; thus we have seen, that brushes always deviate towards deficient atmospheres (115).

568. I once flattered myself, that I could be able to direct, at my pleasure, the passage of the spark BC (Pl. X. fig. 1.) by means of the atmosphere of a charged bottle; but I perceived, that such
atmosphere.

atmosphere, by its increasing the contrary electricity in the rod CD which I touched, served more to favour the direction of the spark towards C, than to make the same move in zig-zag. When I held the bottle pretty near the middle point, between B and C, then the spark, whether I grasped the belly of the bottle, or held the same with an insulating handle, leaped to the hook, concealed itself in it, and thence leaped to the head C of the other rod. From the interposition of the deferent hook, the spark indeed was enabled to run through a longer tract than it would otherwise have done, but then it had a less crooked direction than common, in each of its two partial passages. When I grasped the belly of the bottle, the spark leaped to a still greater distance, and the bottle at the same time was discharged; it seemed as if the hook of the bottle, by its more intensely introducing a contrary electricity in the rod CD, quickened the passage of the spark from B; and reciprocally, the passage of the latter quickened the discharge of the bottle.

SECT. V. *On the division, and on the union of sparks across air, and particularly on the division with circles and rings, observed by Dr. Priestley.*

569. *If a spark, in the act of its passing through a resisting medium, meets with a deferent body, which, not being of itself sufficiently continued, yet is of such a nature, that the spark may explode it into a number of particles, so that the sum of the several intervals that will afterwards take place between those particles, oppose a less resistance than that opposed by the space of air which the spark must otherwise run through, the spark will actually explode that body, and pass, divided, through its particles.*

570. I demonstrated the reality of this law, by placing a sheet of tin in the middle of a quire of paper. I. The spark bores a single hole in those leaves which lie between the conducting bow and the metallic sheet. II. But in those leaves which lie between that sheet, and the ample surface of the coating of the plate on which the quire is placed, it almost always divides itself into two, three, or more small sparks, each of which opens a particular passage to itself through the quire. III. When I afterwards carefully

fully examine those several holes that have been made, I find that they have been made in those places when the leaves, either on account of their thinness, or less compact texture, opposed the least resistance; and that in those places where the largest holes were made, such circumstances plainly appeared to have taken place, as must have made the leaves to yield more readily than in any other part. IV. So that I have at times amused myself in marking out before-hand the places of the holes, either by boring, or by rasping, or wetting, some of the leaves. In order to make the above experiment, I stitch, as I mentioned before, the quire of paper; but then it is necessary to fasten the tin sheet with wafers, else the spark is sure to pass through the holes of the stitches.

571. When the quire of paper has been placed on that surface of the coated plate which is charged by excess; then, at the approach of the conducting bow, it is no longer a spark that divides itself, but a number of small sparks which, from the sheet of tin, gather and unite towards the head of the conducting bow: and the causes and laws of these *unions* of sparks are reciprocally the same with those of their divisions.

572. Nor is it absolutely necessary, in order that the spark may divide itself, that the tin-sheet should be inclosed in a quire of paper. Once, discharging a battery of three large jars through a tin-sheet, I observed that it was not only melted in that place through which the main spark had passed, but that many lucid points had taken place around the main hole; and among these, one had been melted quite through, so that the sheet was bored in that place. Sending, at another time, a like discharge through a simple leaf of paper, I observed that besides the principal hole rent in it, there were several other very small ones; though they were disposed without any apparent order.

573. Hence I concluded, that sparks are aggregations of many other small sparks, and that the copious fire of which the larger sparks are formed, throws around them a number of small ones.

574. It was only after reading the experiments of Dr. Priestley, that I observed the circles and rings that take place on balls of

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glafs broken by sparks. That Gentleman was the first who, drawing sparks from a battery of forty square feet (p. 699.), found that the main spark divided itself into a number of small ones, observing the following order. I. Some of these small sparks bored a number of sharp cavities in the glafs, the aggregation of which formed a circular spot. II. Other sparks bored around the said spot, other cavities that formed a circle, or ring, concentric to it. III. On tin sheets, as they were more easily melted, an additional concentric ring took place. IV. On the compound substance mentioned by Dr. Priestley, which dissolves in boiling water, a third ring was to be perceived. V. The cavities in the middle spot were deeper (to me it has appeared that they are also larger) than in the successive concentric rings. VI. In such metals as are easily melted, the cavities were deeper. VII. The circular spot, and the rings made on different metals were equal, only some small difference in their sizes, arose from the difference in the sizes of the cavities, by which they were respectively formed: I will add here, that I have also observed, that intense sparks (and much more so strokes of lightning), melt a much more considerable part of the surface of a sheet of lead, than of a sheet of tin. Dr. Priestley has made so many other experiments, that he has greatly confirmed my conjectures with regard to the production of *enchanted circles* (See *Atmos. Terr. Elect.* pag. 215.) I will endeavour to repay the service by trying to explain the nature and causes of those circles which he has discovered.

575. I. Though the intense spark sent by Dr. Priestley seems to leap in an indivisible instant, yet its parts in reality succeed one after the other. II. The first part of it rushes, I think, from that part of the convexity of the conducting bow which lies nearest to the coating of the plate, and produces the middle circular spot; but as this same part, by leaping first, strikes against the air with prodigious rapidity, and as the latter cannot immediately give way all around, there results in the fire a momentary increase of density. IV. The remnant of the spark, which is kept back by the foremost portion of it proportionally to the resistance which it meets, is refused admittance by the stratum of condensed air which surrounds this foremost portion, and is thus led to pass

pass through the next stratum; whence results the first ring around the circular spot: an explanation of the same kind may be given for all other additional rings that may take place around the former.

576. From the rapid stroke of sparks against the air, sonorous undulations of the latter must needs arise; hence the loud cracks which strong sparks always produce.

577. Dr. Priestley has also sent intense sparks, like those above mentioned, through air brought to a density double to its common degree, and found that the circles were both less distinct and narrower, than those made in atmospheric air. Air, when denser, must needs oppose a greater resistance to sparks, and thus deaden, as it were, their efficiency; hence I have found, and I think Dr. Priestley has observed the same, that the above circular impressions were deeper when the ball of the conducting bow was in contact with either of the coatings, than when both balls were kept at some distance from them, and the sparks thereby obliged to run through some considerable stratum of air; it indeed seems, that a denser stratum of air weakens a spark in an equal degree, with a higher stratum of the same.

C H A P. II.

On sparks, relative to water, and other liquids.

578. **I** Think that the effects of the electric fire on water, may be reduced to the following principle: I. *The pores of water are of less capacity for conducting the electric fire than those of metals.* II. *Electric fire not only tends to separate the particles of water, and to scatter them into vapour, as common fire does; but it moreover effects this with a rapidity proportioned to the peculiar rapidity with which it moves.*

579. From these principles it follows: I. *That electric fire tends to propagate itself chiefly near the surface of water:* the reason is, that, seeking to diffuse itself through the passage of least resistance within the water, it must move near its surface, because the par-

ticles of the water will there resist less against being separated, and its pores against being opened. *According as the electric fire will be obliged to pass through water, at a greater depth under its surface, the weight of the column which it must then raise, will increase, and of course also the resistance, which it must surmount to force open the pores of the water.*

580. It follows, in the second place, that the electric fire when compelled to pass through a body of water, will exhibit various phenomena, according to the ampleness and the length of its passage, and also according to its own quantity and density. *Let the body of water to be traversed, be both narrow and long; it will obstruct the passage of the fire proportionally to those two circumstances; the reason is, because in consequence of the narrowness of the body of water, its pores will be proportionably fewer; and since the resistance of any medium increases, as I have before demonstrated, in proportion with the length of it, the resistance opposed by the above body of water, will also increase in proportion with its length; hence it follows, that a given quantity of fire will, through a narrow and long body of water, pass divided, and with difficulty.*

581. Let the above body of water remain of the same length, but let its width be increased; then the number of the deferent pores in it will increase proportionally, and the resistance opposed by it also proportionally decrease; therefore, the fire will pass through it more instantaneously and more compact, than formerly.

582. Let the length of the surface of the water be lessened, the resistance arising from this length (which even in metals is pretty considerable), will also be proportionally lessened; consequently the division of the fire will also be less than formerly, and it will pass more united and compact. If the length of the body of water is lessened beyond a certain degree, so that the quantity and density of the fire may divide it with sufficient violence, and scatter it into vapour, this will increase the union of the spark in a peculiar manner: the reason is, because such vapour will, in an instant, expel the ambient air, and effect a kind of vacuum through which the fire will rush the more united and compact; which, at first sight, appear repugnant to the former facts: thus, a little moisture spread over a space of a few inches, will conduct in a very compact

compact united manner, the same discharge of a battery as would, if sent through a fur of water nine inches long, grow divided, and produce in its passage a rattling, or noise more or less continued.

583. But in all these phenomena several various accidents will be complicated with the former, according as the quantity and density of the fire which will be sent through the water, shall vary. A quantity of fire that is only adequate to the capacity of the pores of a given body of water, will pass through the water without sensibly distending its pores, without lightning, without cracking, and without being sensibly retarded. But more dense and more copious fire, in proportion as it will exceed the capacity of those pores, will be proportionally more retarded, will distend more pores, will emit a more vivid light, and produce a brisker cracking; nay, it may produce all these effects, or even explode the water into vapour, through the body of water may be rendered even longer and ampler than formerly.

584. The above observations will account for all the different phenomena exhibited by the electric fire, in his passage through water, or other liquids. Endeavouring, in the chap. VI. of *Artif. Elect.* to confute the common opinion of those philosophers who compared the *conducting* property of water to that of metals, I observed first, that a tube of glass, fresh taken from the dew, transmitted only weak signs of electricity to a Chain, when compared to those transmitted by the same tube, when a very narrow stripe of metal had been pasted to it. Likewise, when the strings with which I suspend my conductors, are in any degree damp, or when the ambient air is damp, I still continue to be able to draw sparks from the conductors; nay, though the rosin cake, on which the FRANKLINIAN iron rod is placed, may have become intirely wet, yet the rod still continues to afford strong sparks.

585. That is to say, the very thin superficies of dewy moisture, which furred the long slender tube of glass, could only conduct little fire to the Chain; the damp strings, and the damp air, could not dissipate the whole fire raised on the conductors; and a great part of the copious quantity of fire which, from the clouds,

clouds, rushed into the FRANKLINIAN iron rod, could not be discharged through the thick fur of water that covered the rosin cake under it ; all which was evidently owing to the pores of water being of a much less capacity for conducting electric fire, than those of metals.

586. In the following chapter I continued the same experiments by attempting to discharge a coated plate through water. I. Through a thin stratum of water, spread on a plate of glass two lines wide, and six inches long, the spark passed divided, with a continued rattling, and produced a light that might easily be perceived in the dark. II. The water contained in a vessel two inches wide, and six inches long, transmitted the discharge to me, with a very weak spark. III. The ample surface of the water contained in a common dish, transmitted to me a stronger spark. IV. When I repeated the experiment by sending the discharges through my body, I was struck by them proportionally to the *union* of the light and of the sparks ; a discharge with bare *rattling*, without sparks, gave me no shock ; such discharges as sparkled more or less, struck me accordingly.

587. Now, if those experiments are combined with those made on lakes and rivers, by Messrs. Jallabert, Monnier, and Franklin (which I have repeated on the surface of the Po), and in which strokes pretty considerable were always obtained, we may conclude this, that in the above mentioned stratum of water two lines wide, and six inches long, the total capacity of the pores apt to conduct the electricity was extremely scanty ; hence the fire became much divided, and produced a rattling noise, and some light, as it distended the superficial pores of the water ; for rattling and lightning always takes place when the electric fire displaces and drives the particles of any resisting medium. But in the small vessel above, in the dish, in rivers, the discharge passed less divided, in proportion as the deferent strata of water were become more numerous ; that is, in proportion as the resistance it met, was become less.

588. In the same chapter I proceeded farther, and attempted to discharge a coated sheet through water inclosed in a slender tube of glass six inches long, and the third part of a line wide ; and I
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never could obtain either sparks, cracks, or light. I inserted brass wires into the tube, and fixed them at a little distance from each other, but it was only when the distance between them was made so short as the third part of a line, that the discharge was able to pass intire and compact, and produced a loud crack, though less so than one produced by a discharge sent through the open air. I. In the interval between the two wires, I saw a vivid light take place. II. The tube was broken, and a piece of it separated, which was half an inch long, and corresponded to the two wires. III. The two main pieces of the tube were flung in opposite directions, and the shatters were flung beyond them.

589. To understand the above facts, we must consider, I. the narrowness and length of the body of water that was inclosed in the tube, and the degree of strength with which the glass tube supported it. On account of the narrowness of the cylinder of water, there were in any section of it made perpendicular to its axis, only a few pores, and these too of a scanty capacity for conducting the electric fire. The length of this same body of water also proportionally increased the resistance of it; because in each section into which we may conceive it to have been divided, an additional resistance was opposed to the spark, which sought to pass undivided: farther, the support from the tube in which this water was contained, did not allow its pores to be suddenly distended, but with difficulty. Now the concurrence of all those impediments must needs have caused the discharge sent in the first instance through the cylinder of water, either to have failed, or to have only been effected in a very divided manner, and without affording either crack or light. II. But when the metallic wires were brought to a small distance from each other, then the distance to be surmounted being thereby proportionally lessened, the spark distended the pores of the water with a force similar to that of the common fire, though with an incomparably greater rapidity; and the water flying into a vapour highly elastic, shattered the tube to pieces.

590. The reason why a spark can pass through a cylinder of inclosed air much longer than any one of water, without shattering the

the tube, is owing to the *compressibility* of air being so much superior to that of water *), and also to the prodigious violence with which water flies into an elastic vapour. Dr. Franklin repeating this my experiment with ink (p. 145.) could not find the least stain left by it, on a sheet of white paper on which he had placed the tubes. One day making use of wine instead of ink, I found the brass wires inserted into the tube, covered with a wet stuff like a fur of mustard; and, another time, as I have related, in the num. 397 of *Artif. Elect.* I made the experiment with a drop of water supported between two iron points that met in the center of an almost solid ball of glass; when the latter, by the sudden explosion of the water, was shivered to atoms.

591. From the numerous experiments I have made on this subject, I conclude, I. That the distance between the metallic wires being given, the discharge meets with a greater resistance against its passing united, and thus breaking the tube, when the latter is closed at both its ends and completely full, than it does either when the two openings of the tube are left open, or when being closed, there remain at both ends, intervals filled by air. II. When the tube is open, it resists the more the spark in proportion as it is longer; that is to say, in proportion as the two cylinders of water, which the spark must drive off on each side of the tube, when it explodes the drop contained between the two wires, are longer.

592. In my Work on *Atmos. Terr. Elect.* I related experiments in which, by making use of very strong sparks, I broke tubes stronger than those above mentioned, and drove their shivers farther (p. 75). Nay, I have made with sealing-wax, a kind of electric gun, with which I can drive a leaden ball into a piece of clay, and make it stick to it. The fig. 2. pl. X. represents the section of an electric *mortar*. I. A H I K is a cylinder of wax cut at a right angle in A H; it has in E F an hemispheric chamber, which is destined to receive the wooden ball P; in D L is the small chamber for the charge. II. I insert into that cylinder a brass wire B L, which reaches the bottom of the chamber destined

* According to an experiment made by Mr. Canton, a given body of water is not contracted by the whole weight of the atmosphere, more than $\frac{1}{48,000,000}$.

to lodge the charge. III. I insert another wire topped with a ball in Q, the extremity D of which gets into the chamber DL. IV. With a pencil I place a drop of water in that chamber, and lodge the ball in the great chamber. V. I lay the mortar on the coated plate, and at the same instant that I approach the conducting bow in G, the spark excited between the two wires BL, DC, makes the drop of water fly into vapour, and drives the ball P to the distance of twenty feet. When I make use of three large charged jars, the ball is driven so far as twenty-five feet distance; if, to the drop of water, I substitute a drop of spirit of wine, the distance to which the ball reaches is still greater; and still more so, if the spirit of wine is impregnated with camphire. It appears, that the inflammability of liquors co-operates with the expansion effected by the discharge; yet, inflammable liquors being of an *insulating* nature, oppose a resistance to the passage of the fire.

563. These ludicrous experiments, nevertheless, explain the most admirable electrical phænomena that take place in nature: such undoubtedly are, I. The general light diffused over houses covered with rain-water, or over the surface of rivers, where a stroke of lightning takes place. II. The violent explosion of liquids effected by lightning. III. The shivering to pieces of the shoes of those who are struck by lightning, which is the effect of the moisture of their feet flying into vapour, as also its rending wet clothes into rags. Nay, from those experiments I have drawn an explanation of one of the most extraordinary phenomena in nature; that is, the fall of stones from the sky. Pythagoras asserted, that they came from the sun, perhaps because he saw them to be either red or black, as if they had been burnt. The circumstances of the fall of one of these stones, accurately ascertained and transmitted to me by Monsignor Fogliani, bishop of Modena, has convinced me, that those stones are driven by lightning, exactly in the same manner as the ball was flung from my electric mortar by the spark; but of this I shall treat at some length in my book on *Nat. Elect.*

594. Proposing now more particularly to examine into the phenomenon of the running of sparks over the surface of water, to

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very great distances, while they open to themselves but very shallow passages through the substance of the same, I think I must make a digression on my former principle, that *the electric fire disposes deferent bodies into its passage*; this digression, though it may be somewhat long, is necessary, because our present subject, and all our subsequent experiments depend on it.

595. This old principle of mine is applicable not only to a few electrical motions, but universally to all such as proceed from a diffusion of the electric fire; the oscillation of electric pendulums, the suspension of corpuscles, &c. but I shall for the present only mention a few of the simplest facts. Rub softly the glass cylinder of the electric apparatus, and then present to the conductor a bason filled with bran, you shall see the stream of electric fire making as it were a long row, or tract, with the particles of the bran, driving and disposing them into its passage.

596. In pag. 52 of *Artif. Elect.* I discharged the coated plate through a candle placed on the plate, and the discharge passed through the flame, lengthening it into the shape of a T, by expanding it towards the head of the conducting bow, and towards a ball of metal placed near it, and raised on the plate; that is to say, the discharge disposed the flame of the candle into its passage, this flame being able to transmit it.

597. I have already related the experiment made by sending a discharge to a greater or less distance, through one or more drops of mercury; the discharge diffuses itself into those drops, and drives them into vapour; part of this vapour, if attentively observed, is seen to rise in the air under the appearance of smoke, and the other part remains imprinted on the glass.

598. A discharge will likewise melt the particles of hard metals, and drives into its passage the deferent vapours arising from them, and also such particles as are not completely melted. Place on the margin of the naked plate HG (Pl. IV. fig. 9.) some filings of iron or of brass, and send through them the strong charge of the coated plate AB; you will find the passage of the fire marked by the smutty track which the effluvia from those filings have left. Lightning has also supplied me with proofs of the truth of the above principle, as we shall hereafter see in its proper place.

599. Dr.

599. Dr. Priestley has thrown an additional light on the truth of this same principle, by several experiments of his own invention, which are contained in his sixth section. He has, for instance, sent discharges through interrupted tracks of brass filings, and through suspended drops of water; and always the brass filings moved forwards to meet the next ones, and thus complete the intervals left between them; and the suspended drops assumed a lengthened shape, striving, as it were to join with each other (after the same manner as I represented, sea trumbs, in my *Work on Nat. Elect.*) and the discharges were thereby enabled to run through spaces of two inches in length; whereas, in common air, they cannot leap to a distance greater than half an inch.

600. To this same principle must be ascribed all the ingenious experiments made by Dr. Priestley in his XI. section, with respect to the sending of discharges along the surfaces of bodies, and especially of water; which is also the object that I proposed here to treat. The result of Dr. Priestley's experiments (except some particular circumstances, which I shall explain in their proper place) may be reduced to the following facts: he sent discharges over the surface of water, of other liquids, of wetted solid bodies, over the surface of a touch-stone, and of a smooth lump of iron ore, to the great distances of seven, eight, or even eleven inches.

601. Now, I do not understand how Dr. Priestley happened not to perceive the dependence of all those facts, on the principle expressed above; and that sparks were enabled to run to such great distances, along the surface of those bodies, by the vapour which they themselves excited as they passed. I imagine his caution against forming any conjectures of that kind, proceeded from his happening not to perceive any vapour arising from a very thin stratum of water, through which he sent a discharge. Mercury, in winter time, is seen to smoke, and leaves marks behind it; common water leaves none, but will do it if it be coloured. With silk strings I have suspended to the arms of a balance, two thin small plates of glass of equal weights, which served as basons to it; I wetted them both with a pencil, so that their weights remained equal to one another; and after sending a discharge through one of them, I always found that

it had lost some part of its weight, and the other preponderated. However, Dr. Priestley's experiments prove this, that a very small quantity of moisture is sufficient to supply the quantity of vapour required to serve as a vehicle to the electric fire. The experiments are also very interesting which he made with a piece of wood that had just been dried; though the wood could at first transmit a discharge, two hours after it could no longer do it; the same may be said of the experiment he made with a bladder; both demonstrate, that little moisture may suffice to enable a spark to run through a *long* passage; but that, on the other hand, this little is necessary.

602. The observation made by the above ingenious Observer, that sparks ran along the surface of a touch-stone, and also along that of a lump of iron ore, in the same manner as they did along the surface of water, has probably been the cause of the particular opinion he has adopted on the present subject. But if he had only examined the places along which those sparks had ran, he would have seen their crooked way marked by a continued track of a cinereous dark colour, which on the touch-stone inclined to reddish; that is to say, the discharge had in its passage exploded a certain quantity of metallic particles which abounded in the above substance, though they were too much parted from each other by terrestrial and sulphureous particles to allow a free passage to the spark; and the latter had been able to run to a great distance, only by its disposing metallic effluvia from those particles into its way, in the same manner as it would have done aqueous vapours, if it had been sent over the surface of water.

603. That, from a touch-stone metallic vapours may be raised by a spark, is very probable, since its black colour is thought to be owing to its being replete with ferrugineous particles. But, besides this conjecture, I have tried to ascertain, whether sparks can actually separate the particles of such stone, whatever they may be, and dispose them into their passages. I filed into dust a bit of touch-stone, and with this dust I made a communication between the plate GH (Pl. IV. fig. 9.), and the coating of the plate AB; and I found, that the spark which I sent through this track of dust thus disposed, had driven it away, and that there only remained,
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instead of the continuous track of dust, small interrupted heaps of it, the surface of which appeared of a cinerous colour. I blew them away, and then saw that the glass indeed was not stained in the very places which those heaps covered, but then the usual stain was conspicuous on all the intervals that had been left between those small heaps.

604. I make, with the same dust, and at a convenient distance from the plate GH, a single small heap of it; and as soon as I present the conducting bow to it, it is dissipated, and the stain takes place over the whole space between the heap, and the edge of the plate. Generally these stains fail in some small interval around the top of the ball of the conducting bow; because the spark chuses in that place to go through the shortest passage which is afforded to it by the convexity of the ball, and in that place only, leaps through the air.

605. From all the above observations it is evident, that an electric spark, when somewhat intense, drives not only water into vapour, but also raises deferent vapours from stones and iron ores, in its passage over those bodies; and thus all the experiments of Dr. Priestley contribute to evince the universality of the property of the electric fire, *of carrying into its passage such particles as, in consequence of their deferency, or aptness to explode the resisting medium in any manner whatever, may facilitate its passage.*

606. But, in order that the extensiveness of the principle may be adequate to that of the subject, we must also lay down the following rule, viz. *that a spark sent over the surface of a body, always will diffuse itself into the substance of that body, in an inverted ratio to the resistance it will experience from it.*

607. A spark, besides the resistance it meets from the superficial particles of the body which it seeks to explode, is also opposed by the air: it even must exert itself the more against the latter's resistance, in proportion as it can less penetrate into the substance of the body over whose surface it runs.

608. Therefore, I. If the body be of itself perfectly deferent, the discharge will wholly penetrate through it, and it will be impossible to conduct it along the surface; hence it is no wonder that Dr. Priestley never was able to make a discharge run along the surface either of mercury, or melted lead.

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609. II. But if the inner substance of the body opposes a great resistance to the spark, and, at the same time, if the superficial particles of that body are capable of yielding to the mechanic action, or to the penetrating heat of this spark, then the latter being repelled by such inward resistance, will run along the surface, exploding on the one hand the resisting air, and on the other hand, penetrating into the superficial pores of the body. And according as the particles of that body will be more easily driven into vapour or dust, and, when in that state, will be more deferent, the spark will run to a distance proportionally greater than it would be able to do, if it had to open a passage to itself through unmixed air, without the assistance of any vehicle.

610. From thence we may explain a variety of facts: a discharge finds much difficulty in penetrating to a great depth, through the substance of water, as is evident from the above experiment of the small tubes; but, on the other hand, it can raise a very expansive vapour from the surface of the same; it can plentifully dispose such vapour into its passage, and make it serve as an agent to drive the resisting air; the consequence of which is, that it can run over the surface of water to the uncommon distance of six or seven inches.

611. A discharge sent along the surface of soft dough, depresses it uniformly all along its passage; because the spark is, in this case, repelled by the air against the soft dough, with a force equal to that with which it itself drives this air, either by its own immediate action, or by that of the vapour which it raises.

612. This may serve to rectify the explanation I gave at pag. 120 of *Artif. Elect.* with respect to the sudden concussion which a man feels in his hand dipped in water, at the instant that a strong discharge is sent along the latter's surface. The quick stroke which the spark, from the repulsion of both the air and the raised vapours, gives on the water, is communicated to the hand through the continued rigid parts of the water, and the hand receives in such case the same kind of shock as a ship does in an earthquake, as I have observed in *Atmos. Terr. Elect.* The above mentioned commotion is, however, different from an electrical shock. I placed my hand, as Dr. Priestley had done before,

fore, under water, in the middle between two metallic rods, which transmitted a discharge through that water; and when this discharge took place, I felt a kind of convulsion in the muscles of my hand; which convulsions were stronger according as I took care to stretch both my thumb and small finger towards the two rods; because, in this case, the discharge passed more united to them from those rods.

613. A discharge sent along the surface of a piece of ice, leaves on it small unequal cavities, which exhibit the same appearance as if a small chain had been placed hot upon it. The hard surface of the ice does not yield to the mechanic action of the spark; or, if it does, it can only do it by being broke in different places: besides, if a discharge can melt the outer surface of crystal (we shall see hereafter that it even makes diamond powder evaporate) it must necessarily melt ice, and the cavities observed on the latter, accordingly exhibit all the usual signs of instantaneous liquefaction.

614. A discharge sent along the surface of snow, dissipates a great quantity of it, making a furrow in it two inches deep, and nearly as broad; but it does not leap to a greater distance than three inches. A spark sent through a body of snow penetrates into it to a great depth, as its particles are much disjointed and moveable (in fact, a spark sent perpendicularly into snow, penetrates into a deeper stratum of it than it would do into water), and thence proceeds that great dissipation, just now observed, of deep particles of snow, when a discharge is sent along its surface. To this circumstance is likewise owing, that the spark can only run to a distance of three inches over the surface of snow, because the small quantity of fire which keeps over this surface, has but little power to raise vapours from it. If you send a discharge through a long track of sweepings or dust from a brasier's shop, the spark will not pass united to any great distance, and will make a furrow along the top of this track; the reason is, that the separations between the deferent particles, and the dust and air that lie between the same, will prevent the spark from penetrating deeply enough into the track; and where it will do it, it will be obliged to drive and scatter away such air and dust, and with these also the deferent particles of the filings.

615. A

615. A discharge sent through a green leaf, tears its surface according to its own direction, and also perpendicularly to this same direction, though, through shorter tracts. The discharge penetrates into the internal moisture of the leaves; and, by driving it into vapours, tears, in every direction, the exterior fibres; an image in miniature, this, of the tearing of large trees to pieces by the violence of lightning.

616. A discharge passes to a certain distance over the surface of spirit of wine, without inflaming it; but if it runs to a greater distance over that surface, it fires it. This experiment suggests, it seems, several luminous ideas. First, it appears, that the facility with which the electric fire runs over the surface of moist substances, is proportioned to the facility with which those substances may be turned into vapour; to this principle, I think must be attributed the effect of the strong spark mentioned in p. 249 of *Atmos. Terr. Elect.* which from the hook of the bottle leaped and struck me at the distance of four inches. And though a spark cannot run along the surface of spirit of wine to any great distance without firing it, yet it most easily moves along it. I understand very well how a spark cannot penetrate into spirit of wine; this is owing to the insulating nature of the latter; but that it so easily moves along its surface is what I cannot so well explain; I attribute it to the facility with which the spark can raise vapours from the spirit of wine, that powerfully drive the resisting air; which explanation is also conformable to what has been above observed with regard to the electric mortar, which could drive balls to a greater distance, when loaded with a drop of spirit of wine, than when it was with water.

617. Dr. Priestley also sent discharges along the surface of animal fluids, and observed, that they produced a louder crack than those on the surface of water, especially on the surface of milk, and whites and yolks of eggs; and I have observed that when, in order to increase a discharge, I make use of spittle instead of water, a more united and louder crack takes place; this I attributed to the greater *volatility* of animal fluids: if I use a drop of lemon juice, the spark passes more difficultly, and produces a less crack. This same principle (the difference in the volatility of substances) will, I think,

I think, also resolve the question why discharges refuse to pass along the surface of recent glasses, though they are in some degree deferent, and consequently ought, it seems, to afford a passage to the fire; why it does not run along the surface of alum, of rock-salt, of blue vitriol, of red vitriol, of polished agate, all of which bodies are in some degree deferent; why it does not move along the surface of dry wood, of dry pasteboard, or of the covering of a book, however polished.

618. The circumstance of the surface being polished is of little importance with regard to the running of the spark over it; it moves freely enough along the surface of a touch-stone, however rough it may be, and on wrinkled flesh, bending its course, in consequence of the repercussion of the air, around all the angles and prominences it meets in its way: the volatility of the particles of these surfaces, joined with the nature of the bodies themselves, which refuse to admit internally any great quantity of electric fire, are the causes of the above effects. Neutral salts are of a very fixed nature, if compared to water; agate and glass have no particles that fire may distend to any great degree; and wood as well as pasteboard, if you take all moisture from them, will be found to be of much the same nature as the above bodies.

619. If a quire of paper has been dried before the fire, a spark will pass through it with more difficulty than formerly; it will pass more divided, and bore a less hole. If you wet it in the place where you intend to present the conducting bow, the spark will make a hole much larger, emit a much more vivid light, and produce a much louder crack; now, sparks will run along the surface of bodies to a certain distance, in the same manner, and in the same circumstances, in which they pass through a moist quire of paper, or any other substance which may be easily and greatly dilated by the action of fire.

620. Dr. Priestley sent a discharge over the surface of a leg of a sheep just killed, and it ran along it to eleven inches distance; over the *medulla spinalis* of an ox, it ran to ten inches distance; does not the reason of these two surprising effects lie in the greater alcalescence, and consequently the greater volatility, that take place in the fluids of dead animals?

On the action of sparks on living bodies.

621. The above observations on the effects of electric sparks with regard to liquids, naturally lead us to examine their effects with regard to living bodies; the following previous observations must, however, be made, viz. that dried fibres, either of animals or plants, the fat substance of animals, and the oily, or the resinous juices of plants, are all of them of an insulating nature. After a long drought, I have found a muscle, and a wooden table, capable of insulating very well, and used them for that purpose, without any farther apparatus; with regard to oils, every one knows, that by excluding moisture from bodies, they render them insulating; and with regard to resins, their electrical uses are so generally known, that to mention any particulars about them, would surely be needless.

622. It, therefore, appears, that the electric fire *does not move through living animals, but inasmuch as they are impregnated with fluids which participate of the nature of water; nor does it shake, distend, burn, and consume the solid parts of their bodies, but inasmuch as it runs through such fluids and explodes them, or gushes out of them against the said solid parts.* We shall see hereafter, that it is one of the characteristic effects of lightning, to dry and stiffen the living bodies it strikes, and to split trees in the direction of their fibres, which is also the direction in which they draw their nutritive juices; we shall also see, that it splits more particularly those fibres, which more copiously draw such nutritive juices, and that it even seems to choose out those trees among others, whose juices are of a more aqueous nature, or which, *ceteris paribus*, are more replete with watery particles.

623. But attending here only to such observations as Art can supply, I shall distribute the matter of this chapter into four principal parts: in the first I shall speak of the effect of sparks on living bodies; in the second place, I shall treat of the effects of electricity, when it is exerted without sparkling; in the third place, I shall offer some considerations on the use which physicians may make of electricity; and lastly, I shall speak of the use which Nature herself makes of it.

SECT. I. *Of the action of sparkling electricity on living bodies.*

624. *A spark runs through that series of muscles, where the result both of the electric capacity, and of the shortness of the passage, is most favourable to it.* Let the man A grasp a bottle with his right hand, and hold a brass rod with the left; let him touch with his rod the naked right foot of a man B; let the same man's left foot communicate through a brass rod with the right foot of another man C; let the same man C, through its left ear, communicate with the right hand of the man D; lastly, let the man D with his left hand touch the hook of the bottle. The man A, whose electric communication is formed by his arms and hands, will be struck through that series of muscles, which is most continued from one of his hands to the other; that is, he will be struck in the muscles of the right hand, of the right arm, of the thorax, of the left arm, and of the left hand. The man B will be struck in the muscles of his right foot, his right leg, and right thigh, and in those which are connected with his left thigh, leg, and foot. The man C will feel the shock in that series of muscles, which from his leg, through which he communicates with the man B, proceeds to his ear, through which he communicates with the man D, &c.

625. In the above instances, the spark, I. *exercises a similar action in similar parts of the body, which are similarly situated, and through which it runs with a similar density.* II. *It extends its action to ampler parts of the body, according as it consists of a more copious and dense fire.* III. *It extends also its action to a greater distance around the said parts, according as its copiousness and density are still more increased.* IV. *And its action is most intense, where, from the resistance it meets, and the narrowness of the passage through which it is led to pass, it is brought to the greatest density.*

626. I discharge a glass, by applying the index of my right hand to one of its coatings, and the index of my left hand to the other coating; the spark strikes both my fingers in the same manner; it also strikes in the same manner my two wrists, my two elbows, my two shoulders. However, in order that the

spark may strike all of them equally, I must place them in similar situations, because a muscle in a state of contraction will always be struck in another manner than a muscle in a state of relaxation: it is needless to add, that the spark must leap in a state of equal density to produce equal shocks; if, for instance, a finger is presented to the coating of a plate, and another to a sharp point annexed to the other coating, the latter finger will receive a more sensible stroke than the former.

627. That the spark extends its action to more numerous parts of the body, according as it consists of a more copious and dense fire, is evident. If with a finger of each of my hands I draw from a plate a very small remnant of charge, I only feel a small shock in those fingers; if the remnant be somewhat greater, my fingers are contracted by the stroke; if the remnant be still greater, I feel the stroke in my wrists and elbows; if I draw the intire charge of a jar, I receive a shock throughout all the ample muscles of my shoulders and thorax.

628. The same holds true, if the spark enters and issues through dissimilar parts. The spark which I excite from the conductor, while I stand on the ground, gives a stroke only to the muscles of my finger, because the spark while it flows through the ample muscles of my legs, or the ample *surface* of the soles of my feet, has no sufficient density in these parts to produce any sensible stroke; but if I keep one of my feet at a distance from the ground, I begin to feel a kind of stroke in the muscles of my other leg, which remains placed on it; and if, being insulated, I present one of my legs to a strange body, the muscles of the same are contracted correspondently to the place where the spark is made to condensate itself and issue; and I feel a proportional stroke.

629. Experience will also make the third point evident, viz. that the spark extends its shock more amply around the part through which it passes, according as it consists of a more copious and dense fire. A middling discharge gives me a shock in my thorax, and in a pretty narrow space between my arms, through both which the fire passes: a more intense discharge strikes me in a greater compass, the shock extends to the muscles of my back, of my belly; and when Dr. Franklin inadvertently sent through both
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his arms the discharge of two large crystal jars, he felt, as himself relates (Franklin's Works, pag. 153.) a general shock from his head down to his feet: the reason of these effects is, that the *wave* of the electric fluid, however rapid, must in propagating itself through the fluids in the body, drive before it the fire which is naturally diffused within them, which fire is compelled to flow to some lateral part, proportionally to the ampleness and density of such wave (Newton Princ. Prop. XLII.)

630. That the spark in its passage through the body of a man produces a greater effect in those places where it is some how brought to a greater density, is evident from our own feeling, and is a necessary consequence of the universal principle according to which the electric fire operates. We particularly feel the effects of the passage of the fire in those parts where the spark enters, or from which it issues more condensed; we feel a particular contraction in the muscles of our fingers, of our wrist, and of our elbow, where the spark from the body of one muscle gets into that of another, through the teguments that are formed around them by the adipose membrane, &c.

631. I think I make suppositions conformable to truth, when I suppose the passage of the spark to be through the muscular system. Whether it be that the muscular fluid of itself admits the spark the most readily, or whether, by having remained longer in the body of the animal, and being more braced and disposed to evaporation, it is rendered more deferent; I do not pretend to say; but it evidently appears from the contraction of the muscles, suddenly effected by the passage of a spark, that this passage takes place through them. Observe with attention, when you draw a spark from the hand, the arm, or the leg of a man, and you always will see that particular kind of motion take place in a muscle, which it is of itself inclined to perform. We, indeed, meet with instances in which sparks, and more so the enormous stream of a stroke of lightning, is partly diffused into some sanguineous vessels; we even sometimes see marks of clotted blood in the smaller superficial vessels of pretty large animals, through the bodies of which strong discharges have been sent; but this does not contradict what we said above, that a discharge prefers to run through the muscular system.

632. To

632. To that end, I have more attentively examined the contractions which sparks produce in the muscles of animals, by repeating on the muscles of the thigh of a strong cock, the experiments which I formerly described in p. 129 of *Art. Elect.* and Signor Buffone, surgeon in the hospital of S. Giovanni, has been so kind as to assist me. We fastened the cock to a chair, its wings were tied to the highest cross-stick of the back of the chair; its right leg was fastened to the lowest stick, a man held its head, and its left leg was left at liberty, for the surgeon to operate on it. The thigh being stripped of all its common teguments, without lacerating its fleshy fibres, I began to send sparks through the several muscles which remained united, and in their natural position, successively. I used my usual jars, which contain at most half a pint; with one hand I held the belly of it, to which was fastened a brass chain; with my other hand, and a stick of sealing-wax, I managed the other end of the brass chain; I presented this end of the brass chain to the extremity of a muscle, and the hook of the bottle to the other extremity; and the passage of the spark being thus evidently determined, I constantly made the following observations, I. Any one of the muscles, when traversed by the spark, was vehemently contracted, so that it always compelled the hand of the surgeon to give way. II. The contraction of the muscle always was attended with a sudden and proportionally violent swelling of it; meanwhile, the places where the membrane which parts one muscle from another, was inserted between them, remained depressed. III. In the place through which the spark ran, the membrane which vested the muscle, from moist and smooth became in an instant dry and wrinkled, the direction of the wrinkles being transversal to the length of the muscle, and a very conspicuous vapour, or smoke, arose over that place. IV. The dryness, the wrinkles, and the smoke, continued after the spark had passed; but these different accidents afterwards gradually vanished, so that the membrane partly recovered that kind of gloss, which is the result of moisture; another spark being sent afresh, the same appearances again took place. V. While one muscle was thus contracted, a kind of general contraction was observable in all the muscles contiguous to it; and there was hardly

hardly a muscle in the thigh that remained in a quiet state, which I have considered as a visible proof of the expansion of the shock into lateral parts. VI. For several seconds after the passage of the spark, small convulsions took place in the muscle, and it was during that time that the above mentioned wrinkles began to lessen, and a new moisture gradually appeared on the muscle.

633. I afterwards got the body of the muscle which stretches itself along the lateral surface of the thigh, and which till then had been let alone, to be separated from the muscles placed under it; and, besides the circumstances observed above, all of which took place in this muscle, I made the following experiment. We parted the body of the muscle from the thigh, by passing our finger under it and drawing it out, and at the same time holding firm the articulation of the leg, so that the muscle could not return to its place; which done, the body of the muscle appeared relaxed, and remained parted from the thigh; but, at the instant that the spark was sent, the muscle violently contracted itself, and was drawn back into its natural place; nor could it be displaced again but by being afresh forcibly drawn out by the fingers: when thus parted again, a new spark again replaced it in its former situation. If one of the sides of the muscle being twisted out, the spark was sent only through the *venter* of the same, the side did not exactly recover its former situation; but if a single new spark was sent along the side, the muscle instantly recovered its natural situation.

634. The surgeon, before he parted this muscle from the others, pricked with his scalpel the muscle next to it, pretty deeply, but for all that no great contraction ensued. Having tortured the parted muscle with a number of sparks, the surgeon cut with scissars some lateral fibres, and the muscle scarcely moved; it seemed as if the preceding sparks had rendered it insensible to the stimulus of the instruments, and fresh sparks alone could contract it. I sent some discharges from the bottle into a wound made with the scalpel; the same contraction took place, and the edges of the wound remained particularly turgid and livid, with some small clots of blood around them.

635. I afterwards sent in the dark several sparks through different muscles, and I could not see them lighten, either at their entering it, or at their issuing from it; I, therefore, concluded, that they penetrated and went through it.

636. From the whole of these observations, I think that several interesting facts are either discovered or ascertained. First, the sudden aridity, and the smoke, which are produced by the spark in the place of its passage, manifest the explosion of the moisture or fluid, with which the muscle is impregnated; which fluid is, of itself, very apt to evaporate and dissipate, as may be observed in animals fresh killed; and the bare rugosity of the membrane is a proof of the dissipation of this moisture or fluid: and is not the contraction of the muscle, so superior to that caused by any incision of its fibres, a proof of the sudden and violent expansion of the said moisture, or fluid, effected by the dense, penetrating, and heating igneous element, which proportionally distends and swells the fibres of this muscle.

637. From these violent contraction of muscles, a vast field is here opened for conjectures about the cause of the voluntary contractions of muscles; but such inquiries are too foreign to our Subject to engage in them at present; I will, therefore, content myself with laying down the following maxim, viz. *that those violent contractions above described, are of the same kind with those produced by the action of the electric fire when it kills a mouse, a bird, a pigeon, a chicken, or even a man, in consequence of its rushing through them with a copious proportional stream; the fire in those instances so much distends and swells the fluid of the muscular system, that it injures the whole system itself, and renders it unfit for performing again its vital functions. I even think that such a stream can kill, merely by causing a damage to the body of the same kind as might arise from a prodigious and sudden weariness (stracchezza).*

638. I. The copiousness of the stream must be proportioned to the ampleness of the system through which it is to run. With a single spark from a coated sheet, I can kill a mouse, a sparrow, &c. To kill a pigeon, one spark is not sufficient; three are requisite, or even more, that succeed each other pretty quickly, and

and before the vital force can repair the injury occasioned by the preceding sparks; I even think, that a man might be killed with multiplied sparks from simple sheets, if they were sent quickly one after the other.

639. II. The particular kind of effect caused in animals, when they receive a killing stroke from a strong spark, is a beginning of a violent contraction, but which does not proceed farther, because the universal contraction which takes place in every antagonist muscle, prevents it. Soon after this beginning of contraction, a more or less violent distension takes place in all parts of the body; when the stream is very copious, the animal dies in this beginning contraction, and the ensuing distension does not obtain; thus we have instances of men struck by lightning, who have remained dead and stiff in the very position they were in when struck.

640. III. When an animal is struck dead by a spark just sufficient to kill it, it appears, that it does not die either in consequence of any rupture of its fibres, or any extravasation of its liquids. In various animals killed by a spark, I never could, though assisted by expert persons (the same has happened with Dr. Priestley) perceive any rupture in the thorax, or any wound that penetrated under the skull into the substance of the brain. In a pigeon killed by a spark, there was indeed a reddish spot in the pericranium, but more internally nothing could be perceived; and there are, on the other hand, instances of men killed by lightning, on whose bodies no visible signs of wounds, or in general, of the least injury were to be perceived; hence it seems, that even when lightning enters so divided, as to leave no marks of injury on the surface of the body, yet, if it enters with such a copiousness that it may contract to a great degree the muscular system, it kills the animal.

641. IV. To this must be added, that with some proper assistance I have restored to life animals that had been struck with sparks just sufficient to kill them. I once sent the discharge of two large crystal jars through the head of a hen; the hen, after successively dropping down its head, and stretching her legs, in less than a minute lay completely motionless, and rose no more.

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I struck in the same manner another hen, rather inferior in size, or at most equal, to the former; she fell as the former had done, but after fifteen or twenty seconds had elapsed, we began to rub her all over her body; when she soon began to raise again her head, contract her legs, and looked around herself, as if she sought for something to lean upon, but could not rise; having opened her beak, we poured drops of spirit of wine into her throat, and she soon rose and lived. Hence it is evident, that electric sparks, or lightning, can kill without any material rupture of organical parts.

642. V. Death may also follow a stroke from a spark, or from lightning, though no such quantity of the vital fluid has been dissipated, as may create a mortal *ficcidity* in the body. Having strongly charged the coated plate B A, I discharged it through a mouse, which was tied to a piece of glass: the mouse was killed; and, as I had weighed it immediately before the discharge (it weighed 7 den. $13\frac{1}{2}$ grains) I found after it, that it had only lost about the third part of a grain. Now, I do not think that such a small quantity of animal moisture was necessary to the life of the mouse; but, on the other hand, if we consider both the enormous dilatation which this small quantity of fluid may have suffered when exploded by the spark (I think it may have been dilated to a volume fourteen thousand times greater than it was before) and the rapidity of the stroke of this spark, we shall not be surprised, that a mortal *weariness* ensued. In this mouse I could not perceive the least sign of any, either internal or external, solution of continuity.

643. It really seems, that a certain degree of that kind of injury, which I express by the word *weariness* (*stracchezza*), is more to be dreaded than any wound, fracture, or burning, from lightning. Four of the eight children who were struck by lightning under the tree of *Monistero*, in the neighbourhood of Mondovì. (*Atmos. Terr. Elect.* p. 240.) had their skin stripped off in different parts; the one, all over one of the cheeks, the other off both the shoulders and a little below his back bone, the third near the back of the neck, where the hair was burnt and his hat bored through, and the fourth, along the arm; but none of these wounds were deep,
nor

nor broader than one inch : they all were seemingly dead, deprived of sense and motion ; but, when carried home, recovered in less than one hour, except him who had been wounded on his head ; who, after several hours, continued to be looked upon as dead ; but his mother having applied her hand to his breast, felt some pulsations of his heart, and upon his being bled, he also recovered. Three days after, I went to the spot to see them ; when, being invited by the promise of some small gratification, they came to the Judge of the place ; but both their look and carriage, indicated a degree of *weariness*, even greater than that of the harvest-men in the *Campagna Romana*.

644. VII. Even the men struck in the dungeon of the citadel of this place (*Turin*), even many of the persons who were struck by that tremendous stroke of lightning at *Villa di Stellone*, have recovered. I think that when the electric fire strikes, united enough, some particular place of the human body, so as to tear off the skin, the vapour which it then raises, can in a great measure conduct the stream along the surface of the body, and thus lessen the internal deadly shock. It seemed to me, that I plainly saw such a deviation of the electric stream take place on a poplar that had been struck by lightning in the plain of Mondovi : its branches, from the middle to the top, had been dried by the effect of the stroke ; with this circumstance, however, that the upper ones remained erect ; the lower ones, which were very long, were partly broken, partly bent downwards, and directed towards a rivulet which ran close to the foot of the tree. It is probable, that the strength of the flash had disposed those boughs into its way, and the moisture that was forced out of them (I was told, that the stroke of lightning preceded the rain) had been sufficient to conduct the fire out of the tree through the air, and thus caused the lower part of it to be spared. But the strong sparks before mentioned, which run with such copiousness and light over the leg of a sheep fresh killed, and the medulla spinalis of an ox, are still more evident proofs of the propensity of the electric fluid to flow along the outside of the body of a man, when it has once torn off the skin, and scattered a deferent moisture over the body.

645. VIII. This hypothesis, that lightning usually kills by its causing a sudden violent *weariness* in the muscular system, is also confirmed by observing that the flesh of chicken killed by an electric spark, eats more tender than usual; the bodies of persons killed by lightning pass sooner to putrefaction; the prodigious contraction of all the fibres in the body of chicken, must render them tender in the same manner as driving and hunting soften the fibres of an ox, or a stag; and thence we may conclude, that the dense penetrating deadly fire, may also raise the liquids in the body of an animal to a great degree of alkalescence.

SECT. II. *On the action of electricity on living bodies.*

646. Though the professed subject of this chapter was to treat of electric sparks, yet, I hope the reader will excuse the following digression about electricity without sparks; its effects are too much connected with the matter of the preceding article, to refer the discussion of them to another place.

647. First, the Abbé Nollet has found, by accurate experiments, that the electricity without sparks, promotes the evaporation of liquids proportionally, not so much to their surface, as to their natural propensity to evaporate: the real fact is this, *the evaporation is not, and cannot be, proportioned to the absolute surface (ceteris partibus) of liquids, but must be, and is, proportioned to the liberty of the surfaces themselves.* By the words *liberty* of the surfaces, I mean their not being counteracted by an electricity homologous to that by which they are themselves animated.

648. I procured small cans of tin, two inches high, cylindric, and eight lines wide; I filled the one with water, and placed it at the bottom of the electric well; the other I left in the open air, and put into it only half an inch of water, and placed it six feet distant from the first conductor; I placed next to it a third can intirely filled with water; afterwards I insulated both the cans and the electric well, and made them communicate with the conductor, through iron wires that touched their bottoms. Now, neither the can placed in the bottom of the well, nor that
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in which the surface of the water was eighteen lines lower than the brim of it, had, in three hours time, lost any thing of their weight, but the cann which was quite full had lost a grain, and even something more of its weight.

649. I. I suspended to the conductor one of the same cans full of water, at the distance of three inches from the inferior surface of it; and to the bottom of this cann, I fastened a plate of tin, one foot wide. II. I fixed another cann on the conductor itself, which was also full. III. I insulated two other cans, at six feet distance from the conductor; the one was on the one side of the conductor, and the other on the other side; and over the one I suspended, at five inches distance from its surface, a circular plate of tin one foot in diameter. The electricity having been continually excited for three hours together, I found that the cann suspended to the conductor, had lost nothing of its weight; that which was placed upon it had lost about half a grain; that which had nothing suspended above it, had lost about a grain; and the last, above which a plate of tin was suspended, had lost a grain and an half of its weight.

650. Hence we see, that the evaporation was nothing where the surface of the water was counteracted on all parts by an homologous electricity: in the cann placed on the conductor, only a little evaporation had taken place, because the atmosphere of the conductor bent itself over the surface of the water in that cann, and lessened the evaporation; in the cann placed at a great distance from the conductor, the evaporation was great; but it was still greater in the cann over which the plate of tin had been suspended, because a contrary electricity was actuated in that plate by the atmosphere of the cann: in consequence of this, a continual dissipation of vapour was effected by invisible effluvia of the electric fire, which, through the plate were enabled to be diffused away: from this same fact, we may also derive a new confirmation of the principle above mentioned, that the electric fire carries and disposes deferent bodies in its way, &c.

651. Thence we see, that *the evaporation of liquids is not always proportioned to the ampleness of their surface*; because, as the electric atmospheres of liquids are much less counteracted by the

the reaction of the atmospheres of the adjacent parts, close to the sides of the vessels in which they are contained, than in other places, their evaporation must, of course, be proportionally greater.

652. A number of other circumstances may also alter the quantity of electrical evaporation. I. A square vessel will, as I have experienced, produce a greater evaporation than a circular one, because the electric fire more easily dissipates through the angles of such vessel. II. The liquor in a given vessel will evaporate the faster, as the height of its edge above the surface of the water, will be in a smaller ratio to the width of it; this is because the electricity of the edge will then counteract the less the electricity which promotes the evaporation of the fluid. With regard to other circumstances, such as the temperature of the air, the dryness, or dampness of the weather, &c. I think it would be needless here to expatiate upon them.

653. Mr. Boze, a great admirer of electric phenomena, once communicated the following observation of his own to the Abbé Nollet: *Electricity* (he found) *makes water run under the shape of a continued brush, from such syphons as, on account of the smallness of their orifices, would only emit it in much interrupted drops.* The Abbé, as a confirmation of this experiment, found afterwards, I. That water may not only be made to spring out of the above mentioned syphons, under the shape of a continued brush; but that the quantity of the water that issued thus, might also be much increased. II. That the quantity of water springing from such a syphon, increased the more (relatively to what it would have done if left to itself) in proportion as the aperture of the syphon was less. III. That electricity can make water spring from such syphons as have too small orifices for any water to issue out of them, even in drops. IV. That when the orifice of the syphon is so wide as to let water flow out of it in a continued stream, then electricity accelerates it but little. V. When the orifice of the syphon is a line wide, electricity neither accelerates nor retards the water. VI. And, lastly, the Abbé concludes with observing, that when the orifice is more than a line wide, electricity rather retards than accelerates it.

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654. From these experiments, the Abbé was led to consider organized bodies in general, as assemblages of capillar tubes, filled with a fluid which endeavours to circulate through them, and sometimes to spring out of them; he, therefore, electrified fruits, green plants, wetted sponges, and found, that they lost more of their weight than similar bodies not electrified.

655. Mr. Maimbray, in Edinburgh, electrified two mirtles, during the whole month of October, in the year 1746, and observed, that they vegetated more quickly than other mirtles not electrified. Encouraged by this discovery, the Abbé Nollet made farther experiments, and found, that seeds rise sooner in electrified vessels, than in vessels which are not. About that same time, Mr. Jallabert, Mr. Boze, and the Abbé Menon, made experiments of the same kind: and, first, Mr. Jallabert observed, that electricity, while it promotes vegetation, promotes likewise evaporation; which he easily ascertained by weighing the decanters filled with water, on which he had placed onions of various flowers, and by comparing afterwards the remaining weights of the electrified decanters, with the weights of those which had not been electrified.

656. From the vegetation of organic bodies, the Abbé passed to experiments on animals, on cats, pigeons, bullfinches, chaffinches, &c. and he likewise found, that electricity always promoted their insensible transpiration; for, those animals who had been electrified, were always found to have lost more of their weight than others had done. Nor did the Abbé neglect to experiment on men; he found, I. That a person of a good age and health, by being electrified during five hours, lost several ounces more of his weight, than usual. II. That the person did not receive thence the least inconvenience, only a kind of weakness, and a better appetite.

657. The Abbé concluded his experiments, with observing, that not only such animals as are electrified, but even those that stand near them, transpire more than usual, and thence he endeavours to confirm his hypothesis of an *affluent substance*; but his valuable experiments had no occasion to be thus connected with an ill-grounded theory.

SECT. III. *On the medical uses of the sparkling, and not sparkling electricity.*

658. Whoever has a mind to see how many attempts have been made to render electricity useful towards restoring, or preserving, the health of man, may read the copious collection of experiments for that purpose, given by Dr. Priestley in his valuable History of Electricity; as to me, if I here offer to say any thing on that subject, which belongs to a particular *Faculty*, it will be rather with an intent to ask the opinions of other persons, than to offer mine.

659. And, first, with regard to that which belongs to the *sparkling* electricity, does it not evidently appear from what has been above said, concerning those who die from a stroke of lightning, that many of them might have been effectually assisted? Now, which method of assistance would have been the best in such case?

660. II. Is the idea of introducing medical effluvia through the skin of man, by the means of electricity, repugnant to nature, either with regard to the effluvia themselves, or to the particular method of introducing them? I am aware, that I can hardly discuss again this question, which is commonly looked upon as having been fully confuted, without exposing myself to ridicule; but why should the laughter of unreflecting men prevent researches, the object of which is to promote the public good. Besides, I do not propose here to revive the old question on this subject, which I confess I always looked upon as not deserving a serious examination; but, my intention here, is to propose a new idea, which a new principle of mine has suggested to me. From the very beginning I was convinced, that medical effluvia could not pass through the substance of glass; from the beginning I experienced within myself, that purgative drugs held fast within my hand, produced no effect; and now, I moreover find, that being inclosed on all sides by my hand, it was impossible that they could be electrified. Nor did I, at any time, entertain a suspicion, that sparks sent through medical drugs, could carry along with them the effluvia
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of these drugs through the metallic conductors. Dr. Franklin p. 83.) made his rubbing cushion draw its electricity through a Chain, which was dipped in oil of turpentine, and received it from another Chain, also dipped in the same oil, through a stratum of that oil, about an inch thick, but he could not perceive, that the least effluvium was carried off by the fire.

661. The only point with regard to which, I entertain any suspicion on that subject, is whether sparks excited immediately from medical substances, can introduce effluvia from them into the skin of those who excite them, either with their finger, or any other part of their body. In fact, it is a constant observation, that sparks drive deferent vapours from bodies, and imprint them on the surface of those bodies to which they rush. I see that Dr. Franklin has, without obtaining any effect, transmitted through his body several discharges of a bottle, within which, instead of water, he had put a strong purgative liquid. I do not know whether he excited the discharges by touching the hook as usual, in which case the experiment would prove nothing against my suspicions, or whether he excited the spark immediately from the surface of the purgative liquid; nor would even this latter experiment, supposing it failed, afford an universal conclusion against my idea, since a man's skin may refuse admittance to the purging particles, in the same manner as it refuses admittance to the salt of sea-water. I do not, however, pretend to affirm any thing with regard to any particular substance; I only inquire whether the tendency of electric sparks to drive deferent vapours into their way, being once laid down as a fact, there would be no substance from which they could, with some kind of advantage, drive vapours into the skin of man. From the following experiment some light on this subject may probably be derived. I place a large drop of mercury in the bottom of a porcellain cup, which communicates with the Chain; I fix two pieces of gold wire equally near the drop of mercury, on each side of it; I constantly touch one of them, and thus cause the sparks constantly to leap to it from the drop; after a number of sparks, I find the piece to which the sparks leaped, dyed with a sky-blue colour; whereas, on the other wire, no such mark is to be perceived.

662. III. The effects produced by the discharge of a small jar on the muscle of the cock above mentioned, clearly shew, that copious electric discharges sent through the body of a man, may produce very bad effects; they may distend too much the fibres, they may *weary* them too much, &c. Such cures, besides, as may be effected by the means of sparkling electricity, do not always depend on the agitation and dissolution of stagnant fluids, merely; they also often proceed from the tone which sparks can give to fibres; they may also be effected by a kind of stimulus, which sparks may introduce into glands, &c. Formerly I used to dry the fissures opened in my fingers by the action of cold, by means of electric sparks; but the experiment related above of the smoke that rose from a muscle through which a spark had been sent, and the aridity which took place on the membrane around it, evince still more clearly the virtue of electric sparks in dissolving and dissipating liquids. The replacing of a muscle into its natural situation, and the restoring it to its former tension, manifest the power of sparks in giving tone to a flaccid fibre. The increased secretion of wax in the ears, caused by electricity, and observed by Linnæus (*Carmichael Tentamen*, pag. 33.) demonstrates also, it seems, that sparks can operate, barely by stimulating those parts of the human body, which lie near those from which they are immediately drawn.

663. IV. Whatever may be the manner of the operation, would it not be better to direct, and confine the passage of sparks, to the afflicted part alone, as I did on the muscle of the cock, without affecting other parts, which have no need of such assistance? Dr. Watson used this method in his celebrated cure of a female child, who was afflicted with that dreadful disorder, an universal *tetanos*; he began with sending sparks through the muscles of her lips, then through those of her neck, &c. But is it not moreover necessary, to send sparks through different portions of the afflicted part, when its extent is somewhat considerable? The spark which, when it went only through the venter of the muscle of the cock, could not replace it into its former natural situation, seems to prove this precaution to be necessary.

664. Having

664. Having received a small accidental wound in one of my fingers, I tried (*Artif. Elect.* p. 128.) to receive through it a few sparks from the Chain, and I felt at every one of them a kind of chillness, similar to the disagreeable impression we feel in the nerves of the face, or of the breast, from the hissing of a file which is driven against the edge of a plate of iron. Now, is not such chillness a warning of Nature, not to expose a naked or lacerated nerve, to the action of sparks?

665. With regard to the medical uses of the not sparkling electricity, since it is generally agreed, that it does not produce any ill effects, and promotes perspiration, nay, since in many cases it has proved beneficial, it must be the care of the professors of the art, to inquire in what disorders chiefly the use of it may be prescribed, and to examine whether it would really be worth the while to establish public places, where electrization might be more easily performed, and with less expence, after the manner Dr. Priestley has proposed it: the pavement of the room, he says, ought to be insulated; and the machine ought to move by the impulsion of water, or of wind. To this I add, that under the room, stoves ought to be placed that might, when necessary, preserve the insulation of the room, and of the air in it; that in order to procure a sufficient electricity to the pavement, and to the bodies of the people in the room, a number of glasses ought to be rubbed at once; the rubbing instruments ought to be improved, and since the amalgam of mercury and tin, by a rapid continued friction, consumes the glass, and renders it rough, and consequently incapable of performing any longer its functions, it would be better to keep the rubbing cushions constantly wet, than cover them with amalgam.

666. But would it not also be a very good precaution, if sickly persons had around them, within their clothes, the necessary electric apparatus; and the moving principle of the electricity, in their own usual free motions? Very fine wool, when softly rubbed with the hand, becomes strongly electrified; why then would it not become equally electrified by the friction it would receive from the whole surface of the body, if it were used in lieu of linen?

667. Even the troublesome sensation of the friction of wool against the skin, might be avoided ; in winter time I wear a waistcoat of beaver over my linen shirt, and when I undress, I very frequently perceive those electrical signs which I formerly described to Signor Vaudania, in *Art. and Nat. Elect.* p. 197. I think that by tying both the linen and the waistcoat to different articulations of the body, the continual motion of it would create a constant electricity, which to the least possible trouble would join the greatest advantage ; the body would, in this case, *supply* the fire which the linen would *give* to the beaver.

SECT. IV. *Of the use which Nature can make of electricity with regard to living bodies.*

668. When I shall treat of natural Electricity, an occasion will not perhaps offer, to treat of the use which Nature may make of electricity with regard to animals, I shall, therefore, treat here of the use which she makes of it with regard to living bodies, universally ; and surely, it seems, that Nature, who leaves nothing unemployed ; and from every element and every elementary force, draws the greatest possible effect for the completion of her ends, must needs greatly avail herself of that most active element, the electric fire, in her extensive operations concerning vegetation, animal functions, &c.

669. In fact, it seems altogether improbable, that the perpetual intestine motions with which all the operations of Nature are performed, should be executed, and that, in the meanwhile, the electric fire, which penetrates through all bodies and is of itself in so high a degree inclined to motion, should remain in a quiet state ; common fire, when compared to electric fire, seems a kind of a tardy, sluggish substance, and of all the elements known to us, light alone equals it. I might add, that as Nature has allotted to all kinds of bodies a certain portion of electric fire, it seems that neither the dimensions, nor the qualities of bodies, can be altered, without altering also the shares of electric fire that belong to them, and without causing the same fire

to move and operate. I forbear here to dwell on all these considerations, as well as omit several others, because, however well-grounded they may appear, yet to our humiliation let it be said, we know of no precise facts by which we can confirm these conjectures.

670. We have not yet been able to perceive the action of the electric fire, in any effervescence, or in any coagulation, or in any evaporation. In the gem *Tourmalin* alone (whatever philosophers may have said with regard to electrifying sulphurs and resins, by heating them) it has been found possible to actuate the electric fire by the help of the common fire. In all other alterations, whether natural or artificial, of the state of the electric fire (the case of the electricity in the atmosphere excepted) it seems, that it always finds a place where to equilibrate itself, in the very same instant when it loses its equilibrium in another; it seems that the one parts of such bodies as have their electric fire unbalanced, supply this same fire with a proportional capacity to lodge itself in, as soon as the capacity of the other parts becomes proportionally lessened.

671. I have sometimes had a suspicion, with regard to the attempts that have been made to excite electricity by the means of evaporation, that the observations had not been made at distances sufficiently great from the efficient principle of the evaporation. Signor D. *Ceca*, a most worthy priest in the *Superga**, who, encouraged also by the Signor Abate *Malingri* of Bagnolo, has for these two years past communicated to me several observations he has made with regard to the atmospheric electricity, has, at my request, made also the following trial; he raised a most copious smoke under the long and lofty iron wire, which, from the cupola of the temple, communicates with the parish church; but no

* The *Superga*, a magnificent convent and church, situated on the top of a mountain, about four or five miles distant from Turin, from whence it is easily discovered in clear weather. It is now the usual place of the sepulture of the kings of Sardinia; it was built on the same spot from whence Victor Amedea, saw the battle gained by prince Eugene under the walls of Turin, in the year 1706, in consequence of a vow he made, in case the event of the battle proved favourable to his cause.

sensible alteration took place in the small electricity by which, as it was serene weather, the said wire was of itself animated.

672. With regard to atmospheric electricity it appears manifest, that Nature makes an extensive use of it for promoting vegetation. I. In the spring, when plants begin to grow, *temporary* and electric clouds begin to appear, and pour frequent electric rains; the electricity of clouds, and of rain, increases afterwards in summer, and continues to do so, till that part of autumn in which the last fruits are gathered; so that it appears, that the electricity which obtains in clouds and rain, when carried to a certain degree, serves to promote, with regard to vegetation, the effects of common heat.

673. II. It even seems that electricity successively supplies common heat itself, with that moisture, by the help of which it actuates and animates vegetation; which, if heat acted alone, would inevitably be stopped. In fact, it is the electric fire that gathers the vapours together, forms clouds with them, and afterwards dissolves them into rain; it is the same fire, therefore, that supplies the earth with the nutritive moisture which is necessary to plants; and this moisture, by melting the terrestrial saline particles it meets with, by diffusing them along with itself into the inmost pores of plants, causes them to grow and vegetate with such admirable incomprehensible regularity.

674. III. The common saying of countrymen, *that no kind of watering gives the country so smiling a look as rain*, may be explained on the same principle. The rainy clouds, by extending their own electric atmospheres to plants, dispose the pores of the latter to receive with greater facility, the liquid which is soon to follow; and the succeeding drops penetrate into them the better, as every one carries along with it a portion of the penetrating dilating element.

675. I know that the regular distribution of water which is made by rain, also contributes to render it particularly useful; it even seems to me, that to each season belong kinds of rain more or less lasting, more or less sudden, and falling in larger or lesser drops, according to the different kind of vegetation which, in every season, are to be promoted; now, do not all these differences

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chiefly proceed from the different degrees of the electricity which such rains distribute, or rather accompany. I have the knowledge of several facts, with which I propose, in time, to increase the probability of these my former conjectures.

676. Besides, the mild electricity by excess, which, as I have observed for these many years past, constantly prevails when the weather is serene, certainly contributes to promote vegetation, in the same manner as experiments have shewn us, that this is likewise the effect of the artificial electricity *without sparks*. And is it not likely, that the former kind of electricity promotes vegetation still better than the latter can do, since Nature increases it and lessens it, in such circumstances, and at such times, as particularly require it.

677. The influence of electricity on living animals is not so manifest, nor can it be so immediate. We cannot, indeed, doubt but animals, birds for instance, indicate the changes of weather. When I have leisure enough to visit the electric observatory which I have established in the gardens of the *Valentino*, I am constantly informed of future changes in the state of the weather, by herons which I observe flying from north to south (*Atmos. Terr. Elect.* p. 268.) and thus I see the prognostic of Virgil verified. *Notasque paludes deserit, atque altam supra volat Ardea nubem*; Nature continues to be the same, and consequently observations are also the same in different ages. However, it is a difficult matter really to distinguish whether such signs from animals, proceed from any particular stimulation exercised on their fibres by the atmospheric electricity, at the time when an alteration of its state takes place, or from the joint alterations of the degree of the dampness in the air, of the winds, and of the electric state of the clouds; which are causes, no doubt, sufficient to determine in different ways the feelings and motions of animals.

678. A greater and more regular connexion seems to take place between certain peculiar sensations which some persons experience at certain changes of weather, and the state of the atmospheric electricity which causes such changes. Such is the case of which Signor Mazeas sent an account to Dr. Hales (see Dr. Priestley's Work, pag. 414.) of a person who was particularly subject to epileptic fits when claps of thunder took place; but
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though this person assured him, that the fear of thunder was by no means the cause of his disorder, and that he felt a real connection between the above phenomena and his epileptical fits; yet, I must still confess, that I do not understand how an alteration in the atmospheric electricity can produce such effects.

679. A single consideration more with respect to living bodies in general, I shall add to those I have offered the reader, which is, that both in animals and men, that kind of motion is found to take place in a very eminent degree, which is apt to raise an electricity. From the friction that takes place between two bodies, of which the one is insulating and the other deferent, or also between two insulating bodies that are of a different, or even of the same nature, but of which the one suffers a greater friction than the other, from such frictions, I say, a greater or less electricity is excited; now, why should not the strong perpetual friction exercised by the blood against the venal, or arterial vessels, also excite electricity? Why should not a small globule of blood, while it rubs against a given part of an artery, diffuse into it some of its own electric fire, or receive some new portion from the same? I admit, that for all this no visible electricity will take place in the man, nor will any electric atmosphere be formed around him, since through the deferent moisture in his body, the electric fire will continually circulate towards the place of the friction, there to re-establish the equilibrium; but then it must be observed, that I do not mean to speak of an electricity that is accumulated, I only speak of an electricity that is simply excited; certainly, when I rub a cylinder either internally coated, or wet on the outside, a certain quantity of fire passes from my hand to the cylinder, though, either the internal armature, or the external moisture, are perpetually restoring the equilibrium which the friction continually tends to alter; and I really think, that such a perpetual excitation, as mentioned above, both takes place, and produces very beneficial effects, in the animal œconomy.

680. I am much inclined to conclude this chapter with laying before the reader the extensive hypothesis, or rather conjecture of Sir Isaac Newton, which I already mentioned in page 116 of *Art. Elect.*

Elect.) when I applied to the electric fluid what this great philosopher said of an æthereal medium (*Newton, Opt. quæst. xx.*) May not the sense of sight consist in the vibrations of this medium, which are excited in the bottom of the eye by rays of light, and propagated to the place of *feeling*, through the solid pellucid, and uniform capillaments of the optic nerve? May not the motions of animals, continues he, be likewise produced by vibrations of the same medium, which are excited by our *willing* faculty, and thence through solid, pellucid, and uniform fibres of nerves, are propagated to our muscles, which they either dilate or shorten?

681. The duration of *vision*, otherwise that lasting impression made on our optic nerves by external objects, which continues to be felt though the objects themselves are removed, may also, I think, be explained by the *phosphoreity*, which is found to be proper to electric light, as well as to common light; so that the electric fluid harboured in the substance of the organs of sight, and vibrated by any light whatever, produces that durable sensation we mention, in the same manner as if it were produced by the action of common light.

682. The velocity with which electric fire moves, and the facility with which it penetrates into bodies, may account for the velocity and ease with which the impressions which it produces on our organs, are transmitted to the *sensorium*. Our feeling no irritation when our natural sensations are affected, may partly be owing to use, and partly to the extremely little density required for producing those sensations.

683. But, however laudable conjectures like these may be in themselves, however useful to excite our curiosity, and support our patience in the investigation of new facts, as well as increase our industry in trying new experiments, yet, I think, they would become blameable, if we suffered ourselves to be led by them into any settled and general conclusions; and the more so, since our natural laziness and vain-glory inclines us to rely on our speculations, rather than make them depend on such consequences as result from facts, which are always difficultly ascertained, never

are investigated but with great labour, and can only afford limited information.

C H A P. IV.

On sparks relatively to fossils, especially.

684. **A**S soon as I had read and repeated the noble experiment made by Dr. Franklin, of sending an electric stroke through a narrow stripe of metallic leaf, inclosed between two plates of glass (Franklin, p. 67.) I became sensible of the law which determined the place where the fusion was to take place. In the first place, I observed the different density and ampleness of the stripe in its various parts, before the stroke was sent; and I found the stripe melted in that part where I had observed the greatest result, both of rarity in the metal, and narrowness in the stripe (*Artif. Elect.* p. 134.) II. I cut the stripes into different irregular shapes, and the fusion always took place where they were narrowest. III. I cut two stripes, the one into a rectangular figure, the other into an acute triangle, and placed the point of the latter in contact with the side of the former, and I always found that the point of the triangle had been melted in preference to any other part (*ibid.*) I concluded the experiment with reducing the above law to the universal principle, that the activity of the electric fire is proportioned to its density; in fact, it must needs be brought to a greater density in those places, where it is compelled to pass through a less number of deferent particles.

685. Dr. Franklin in his answer to Mr. Kinnerley, of the 20th of February, 1762 (Franklin, p. 413.) has expressed more minutely the truth of the above law, relatively to his own experiment; which experiment alone would suffice to render his name immortal, since he has essentially shewn by it the connection of the effects produced by Nature, and of lightning, with those

those produced by Art. A piece of tinfoil, says he, three inches long, a quarter of an inch wide at one end, tapering to a sharp point, and fixed between two pieces of glass, having had the electricity of a large glass jar sent through it, received no injury in its broadest part; towards its middle it appeared melted in spots; where it was narrower, it had been quite melted, and about half an inch of it, near the point, had been reduced to smoke. I usually repeat the same experiment, by placing the stripe of tinfoil *fe*, which is narrowest in the middle, between the two pieces of glass *ha*, *de* (Pl. III. fig. 15.)

686. By the above law we are also undoubtedly to explain the fine experiments made by Dr. Priestley with his brass chain. The doctor has filled a whole section with analysis of most ingenious experiments on the present subject; and yet, he has concluded it with saying, that, with regard to the causes of the phenomena, he has been able to make no conjecture worthy being communicated to the public; and, indeed, this is too much the case with us experiment-makers, who are apt to go through intricate mazes in search of what stands just before our eyes. As for me, as I was so fortunate as to discover in the beginning the above law, it has saved me much trouble: I stitched a very thin small chain of iron, eight inches long, on a sheet of paper, and sent through it the discharge of my plate. I then saw, I. That smutty spots had been left, not in these parts of the paper which the chain touched, according to Dr. Priestley's account of the fact, but in those places where the rings of the chain touched each other. II. I examined all the rings in the points of their reciprocal contacts, and found them all more or less melted in those places: without the assistance of a lens, I could perceive bright points on them; and with a lens, I perceived manifest signs of a sudden fusion. III. With a soft file I made filings of metal, and placed a few of them, both on the naked margin of the plate *GH* (Pl. IV. fig. 9.) and on a piece of paper annexed to this margin; and sending the discharge of the plate through these rare particles, I obtained marks on the plate and the paper, exactly like those I had before seen, after sending the discharge through the small iron chain; therefore, the cause of the spots observed by Dr. Priestley, lies in

the fusion that takes place on the rings of the chain, where the fire is compelled to condensate itself; that is, in those parts where the contact between the rings takes place.

687. All the other effects observed by Dr. Priestley, exactly correspond with the causes assigned above. I. Even the larger chains used by Dr. Priestley, must needs have given some smoke, and produced spots like those above mentioned, since the contact between the rings of those chains, though they were larger, was nevertheless effected by only very few points: to this it is important to add, that Dr. Priestley employed discharges from very considerable batteries. II. A discharge through a continued equable brass-wire, ought to leave no spots, since there was through its whole length a sufficient, uniform, capacity. III. It was very natural, that the chain used by the doctor, should lose so much as half a grain of its weight, since part of its substance had been melted and driven into smoke. IV. On the other hand, the diminution of the weight of the chain, ought not to have been greater, though the chain was longer, because the resistance the discharge met with, and consequently its division, increased proportionably to the number of the rings through which it had to pass. V. When the chain was left at liberty, and simply placed on the paper, its rings ought, as they did, to move forwards and get into one another, because the vapour from the melted metal must needs have parted and driven the rings from each other. VI. The passing spark ought to lighten mostly between the rings; that is, in those places where it was compelled, by the narrowness of the space, to condensate itself. VII. When the chain hung vertically, the light must needs, from the uppermost rings, have grown gradually less, the weight of the lower rings rendering their mutual contact more complete and *ample*. VIII. The paper, even where it was somewhat distant from the chain, which was horizontally stretched over it, may very well have been stained with smutty spots; because, as we shall see hereafter, melted metallic particles, and the smoke that rises from them, may be driven to pretty great distances.

688. One of the experiments of Dr. Priestley has, I guess, particularly contributed to conceal the truth from him. Having placed

placed a part of his small brass chain on three half-crowns, he found that the latter had been melted in the points of contact; but it was very natural that the discharge should prefer to pass through the three capacious pieces of silver, and leave, along their whole extent, the small capacity of the chain; and in those places where, from the chain, the discharge got into the silver pieces, as well as in those where, from the silver pieces, it got back into the chain, it must needs have melted them, in the same manner as, in the former experiments, it melted the rings.

689. When Dr. Priestley held a part of the chain on his hand, the latter was stained in the same manner as the sheet of paper had been, and he only felt a great heat from the chain, which lasted but a short time. This was because the discharge did not leave the chain to get into his hand, which was of a much less deferent nature, as it had done with respect to the silver pieces, which were equally deferent with the chain, and more capacious; if the discharge had really got into his hand, it would undoubtedly have burnt it, and not barely stained it with the vapour of the melted metal.

690. I shall conclude with an experiment of Dr. Priestley, which, in my opinion, completes the demonstration of the explanations I give here; it consists in placing the chain through which the discharge is sent, within a tube of glass. Dr. Priestley has observed, that four rows of spots took place within the tube, in the same manner as if they had been effected by four chains. I have repeated this experiment: with short sticks I fastened a small chain within the orifices of the tube of glass I used; I then sent through it the discharge of two large jars of crystal, and I obtained the above four rows of spots: the bare sight of which pointed out to me the cause from which they proceeded; every stain directly corresponded to the middle point of the angles which the rings formed between themselves; and the vapour which arose from the metal that had been melted in the common *apex* of the four angles formed by every two rings, that is in the common place of contact between them, had been thrown in four opposite directions.

691. This fusion of metals in their common points of contact, is no more than a particular instance of the universal law formerly expressed,

pressed, which is, that a spark always melts metals in those places where it enters into them, and gets out of them ; or again, it is only a consequence of the still more universal law, that a spark operates on each substance proportionally to its own density, and that it grows condensed proportionally to the narrowness of the passage through which it is compelled to pass. When a very copious discharge is excited, both the coating of the plate, and the conducting bow, are melted in several small points of their surface ; if the discharge be sent through a quire of paper within which a leaf of tinfoil has been placed, the latter is melted the more completely, as the discharge, in this case, rushes more united from the leaf to the conducting bow ; but if two or three holes are bored in the quire, then the fusion becomes less, because the discharge passes more divided. If you send a spark through the point of a large needle, perpendicularly fixed on a metallic surface, a longer part of it will be melted, in proportion as the spark will be more united and condensed ; and when I observe the effect of lightning, I can always find marks of its passage through bodies, both in the place of its entrance, and of its getting out, and in general in those places where it has flowed through very narrow parts of the said bodies.

692. But in order more completely to understand this law of the places of *fusion*, two others must be added to that of the *condensation* being in an inverse ratio to the sections of the bodies, through which the fire is to pass. The one of these laws is, that the resistance increases proportionally to the length of the medium through which the passage is effected ; this law I have mentioned in a former place. The other law, which I do not know that any body else has mentioned, is, that the resistance opposed to a spark by a body through which it is sent, is greater in proportion as the place of the passage of the spark, lies deeper in the substance of the body, under its surface.

693. To ascertain the truth of the first law, I placed a stripe of gold or silver leaf, one line broad, between two pieces of glass ; the discharge of such a coated plate as that which I commonly use, melted it only in a few places, and a great remnant of charge was left in the glass ; when the stripe was shortened, and its extremities,

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at the same time, were made wider, so that its former width of one line only obtained in a space of a few lines, the discharge was then much louder and more perfect, and the fusion more complete; which proves the truth of the above assertion, that the resistance which sparks meet, is greater in proportion as the length of the narrow passage left to them, is greater. Dr. Priestley had before made similar observations; with his large batteries he could melt an iron wire, three, six, or nine inches long, according to the strength of the discharge; but could only make red-hot a wire of a greater length.

694. The second law, that the resistance is less, in, or near, the surface of deferent bodies, is entirely consistent with reason. In fig. 7. and 8. of Pl. V. two sections of a cylindric body are represented; the one perpendicular to the axis, the other parallel to it; the direction of the small white lines express the direction of the pores through which the electric fire is to insinuate itself; now, when the stream of it will be so copious as to make it necessary for it to distend those pores, it is evident, that it will easily effect it near the surface, because the solid parts of a body are connected with a successively less number of other particles, in proportion as they are placed at a less depth within the substance of the same, &c.

695. This explanation is the same with that given above, why strong copious sparks almost entirely flow along the surface of bodies which are imperfectly deferent. Metals are the most deferent bodies we know, yet, if we lessen their dimensions, and increase the sparks sent through them, they become in some degree *resistent*, and a greater portion of the fire is then obliged to flow along their surface, and effects a greater alteration in their superficial parts.

696. In fig. 6. Pl. IX. I represent the extremities A *a*, B *b*, of two brass rods, rounded in *a* and *b*, and bored along their axis, though somewhat obliquely, from *a* to E, and from *b* to F; I insert into these two holes an iron wire, the eighteenth part of a line thick, and fasten it by twisting it round the said rods in E and F, so that the portion *ab* be three, six, eight, or more lines long; I surround both the wires, and the extremities of the rods, with
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melted sealing-wax, which I pour around them ; I send several successive sparks from my coated plate through the wire, when I see flashes of light take place within the sealing-wax, and this light increases according as I send a greater number of sparks. After eight, or more discharges, I break the cylinder, and find the surface of the wire black and rough ; I scrape it with my nail, and a dust falls on a leaf of paper, which has all the characteristics of scoriæ of iron, and are, I find, intirely similar to those which may be taken from the remains of the iron wire, which was struck by lightning at the *Superga*. Even when I send through a similar wire, though not inclosed, a discharge less than what is requisite to melt it, I find that it is coloured blue, and that it is blackened, when the discharge is somewhat intense ; now, the blue colour indicates only a small degree of alteration of the superficial parts of the metal. When the wires have been blackened, and the dust around them scraped off, they appear bright underneath, like the iron wire at the *Superga* ; it seems as if a part of the phlogistic driven from the metal, had been fixed on its under surface.

697. The fusion of stripes of metallic leaves between pieces of glass, proves the same things : in proportion as the glasses are less pressed against each other, stains from gold or silver leaves, consist of longer tracts, or stripes, perpendicular to the direction of the passage of the spark.

698. But let us now treat more accurately of the effects of this fusion, or alteration whatever, in metallic stripes. First, a spark when it melts such stripes, from *deferent*, renders them *insulating*. Send, as in numb. 693, a strong discharge through a long and uniformly narrow stripe of gold or silver leaf, the crack, as was said above, will be weak, there will be a great remnant of charge, and though you present afresh the conducting bow to the plate, this remnant will almost intirely refuse to pass. If you charge again the plate, you shall find that the same stripe can only transmit a still smaller portion of the charge, so that the remnant of it might now very easily melt another stripe ; the meaning of all this is, that the particles of the stripe which have been turned into scoriæ, are no longer deferent, and that the whole stripe is become in great measure insulating.

699. From this principle must be explained the impossibility of sending a discharge through the stains from metallic particles, that take place on a plate of glass; a fact, which has perplexed Dr. Priestley. These stains are become insulating, exactly in the same manner as the gold or silver stripes above mentioned.

700. Another effect of sparks with regard to metallic stripes, is to drive their particles into the very substance of the glass, which latter it really melts. I have a visible demonstration of this fact in a cylinder of solid glass, an inch thick, nine inches long: its use was to fix one of the extremities of the long iron wire with which they used, at the Superga, to observe the state of the atmospheric electricity, and it was covered with a cap of tin sharpened at its top. Two sparks of the intense lightning, which in last September struck this apparatus, flung from the cap two pieces of the metal, and drove them into their way along the cylinder of glass, where they imprinted them on the surface of it, at the distance of about two inches and an half from the center of the cap; those stains from the metal are very conspicuous and bright, and are imprinted on all those places of the cylinder on which the spark as it were rested itself: those places are indeed very remarkable; the glass is melted to a sensible depth, and the waves that manifest such fusion, proceed, gradually diminishing, to the spot, where the melted metal is fixed into the glass. Each of these spots resulting from the metal and glass melted together, is disposed in a longitudinal direction, on the surface of the cylinder, and they are about five lines long, and three or four wide.

701. Every body may, by using strong discharges, ascertain the truth of these facts. I have already observed, how, on the surface of a piece of glass, I fix filings of metal well preserved and whole, which would not be possible, unless the glass were superficially melted; besides, if you examine with your nails or fingers, the place where the narrower part of a metallic stripe has been melted by a spark, and driven into the glass, you shall find the latter somewhat hollow, and sensibly rough.

702. It is, therefore, by no means surprising, that the metallic particles thus driven into the glass, resist the action of aqua fortis, or aqua regia: they are driven to a sensible
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depth into the glass, and are coated and protected by a kind of vitreous fur or veil. It seems that the above particles, when entering into the glass, draw around them the fluid particles of the latter's surface, and are coated by them in consequence of the remarkable chymic affinity between glass and metallic substances.

703. But since this heating virtue or force of sparks is sufficient to melt parts of plates of glass, why could it not shatter to pieces the plates themselves. Dr. Franklin, the inventor of this experiment, first saw plates of glass reduced by the effect of a stroke, to a kind of dust like coarse salt, when those plates had been first strongly pressed against each other in a press. I observe, that plates very seldom remain entire, when I have fastened them to each other with strings, and compressed them with weights. As Dr. Franklin used metallic stripes uniformly narrow in their whole length, the glass plates were uniformly broken throughout the length of the stripes; as to me, as I commonly use stripes made narrow only in some small part of them, I obtain more expressive fractures; that is, the plates are shattered into four, five, or six sharp pieces, which exactly converge towards the point where the stripe is narrowest. I have, among others, a plate of glass on which are four fissures, which run from the above point to the very perimeter of the plate, the whole plate having remained united; it affords a demonstration, that pieces of glass are broken in that place where the fire is most condensed.

704. In order to shew that moisture is not necessary to enable a spark to break a sheet, or piece of glass, as it was for breaking the small tube of glass mentioned in a former chapter, I very carefully dried and polished the pieces of glass I used; I warmed them, and placed brass leaves between them fresh taken from the book; and always, when I had previously taken care to tie and press them, I found them broken.

705. To avoid the danger of having any unctuous or moist particles adhere to the brass leaf, I put instead of it, a few very fine filings between the pieces of glass, and in this case also they were broken.

706. Not-

706. Notwithstanding the precautions just now mentioned, I was still afraid lest some unperceived moisture had been the real cause of the above phenomena, but the following accident has removed my suspicion; and, at the same time, has explained to me the reason of all the above facts. Having placed a stripe of gold between two pieces of glass in the manner mentioned above, and sent a discharge through it, I saw one of the two pieces marked with fissures; having loosened it, I found that the fissures only obtained on the outer surface of it, and did not reach the inner surface; whence I concluded, that the heat communicated by the spark to this inner surface, had, by dilating it, prevented its being broke; and as the heat had not yet reached the outer surface (it must be remembered that glass which is not pervious to electric fire, is little more so to common fire) it remained in its usual state of stiffness and contraction, while the other surface of the piece of glass was dilated; and thus it was broken. Fig. 14 of Pl. IX. represents the plate with the fissures in its outer surface, the points indicate the place occupied by the gold leaf, close to the inner surface.

707. The third visible effect produced by sparks, is to alter the colour of bodies, I. In holding the piece of glass against the light, the stain left by gold appears of a purpureous colour; in examining the same directly opposed to light, it appears of a golden purpureous colour; the golden colour is much dilated, and somewhat bright, yet it prevails on the purpureous dye; but if the other surface of the piece of glass be inspected, the purpureous dye prevails on the golden colour; on whatever surface of the glass the stain be examined, a grey bottom is always to be perceived through the purpureous or golden dyes.

708. By holding the piece of glass against the light, in order to observe the stains made by melted silver, they appear of a colour which is between yellow, green, and coffee; in looking directly upon them, they appear of a grey cinereous colour, but with bright white points; in looking on the other surface of the glass, they appear of the same colour as mother-of-pearl.

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709. In holding the piece of glass against the light, the stains made by gilt copper, appear composed of red points, like minium, spread on a grey bottom; in looking directly upon them, you perceive a mixture of a grey colour, with red points, somewhat bright, but rarer than in the former cases; when the stains are observed from the other side of the plate, the red colour predominates.

710. In examining through the glass, stains made by melted tin, they appear of a dirty cinereous colour; being directly looked upon, they appear of a deeper cinereous colour, somewhat bright; being examined from behind, this brightness disappears.

711. I have described the above appearances in the manner they were effected by the discharge of my ordinary coated plate. Sparks more or less intense, I have observed, produce a few differences, but they, however, do not introduce any material change into the appearances above described.

712. From these facts we may, I think, conclude, that the metallic particles contiguous to the glass, suffer, in consequence of this contiguity, a more perfect fusion; this appears evident from the purpureous colour perceived when a gold stain is examined from behind,—from the colour of mother-of-pearl, perceived when a silver stain is observed from behind,—from the more intense and copious red colour, which is to be perceived when a copper stain is observed from behind,—and from the vanishing of the bright points on a stain produced by tin, when observed likewise from behind.

713. It results, in the second place, that especially in stains made by silver, copper, and tin, a colour rises which has a mixture of a grey colour in it; I say, *especially*, because even in stains made by gold, I perceive, when I look directly upon them, somewhat of this grey colour mixed with the rest; and it is by observations similar to this, that I was led to say (pag. 134 of *Art. Elect.*) that stains of various metals had their bottoms of pretty much the same colour.

714. And now thinking, that the affinity of glass with metals, might perhaps contribute to form the colour of such bottoms, I thought of comparing stains made on glass, with those made

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on paper. I adjusted, as formerly, stripes of different metals between pieces of glass, but, between one of the pieces of glass and the stripe, I inserted a sheet of paper, and thus obtained very equal stains, both on the glass and the paper; here follows the description of the stains which constantly took place on the paper.

I. A stain made by gold, was grey and purpureous. II. The stain made by silver, grey and cinereous. III. That of tin, of a grey ferrugineous colour. IV. That of copper, cinereous grey, much like that produced by silver. After these observations, I think I may again propose my former question (*Art. Elect.* p. 135.) Is not this grey colour arising from the fusion of all metals, a sign of some constituent principle common to them all? Are not the different colours perceived in the stains from different metals, signs, or symptoms, of the different substances, or principles, of which they are formed?

715. The fourth visible effect produced by sparks on metallic stripes, relates to the distribution of the latter's melted particles. Universally, in gold, silver, copper, tin, the melted particles are distributed into furrows, or dashes, which are transversal to the direction of the sparks, so that there is an alternate series of lines, which are more deeply coloured in consequence of the particles of the metal being more copious, and more completely melted, and of lines in which such melted particles being more rare, the glass has been left more pellucid. II. These alternate dashes, or lines, are thinnest in gold stains, so that to the naked eye they appear as if they were one continued stain, but with a lens they may be easily distinguished, except in those places where the fusion has been very copious; the same may be said of the alternate dashes in silver stains: in copper stains, the dashes are more discernible; in stains from tin, they are thicker and more apparent. But, besides those *primary* dashes, which we might call simple dashes, there are others that are thicker, and, as it were, composed of several simple ones. This observation serves more amply to confirm my conjecture, exposed in num. 566, that sparks have alternate places of greater and less densities, according as the resistences which alternately, stand firm, and yield, occasion it: universally, sparks melt bodies more completely.

ly, where, from more copious deferent particles, they rush into scantier ones; and, likewise, where they gather themselves into a less space, in order to rush into a wider system of deferent particles; thus, a metallic stripe is melted more completely, in that part of it which is joined to that which grows wider at once.

716. To the subject of the *distribution* of the melted parts of metals, belongs also the consideration of the lateral smoky effluvia from them, which lie transversally to the passage of the spark. In gold and silver, these tracts are seen as if covered with a kind of exhalation. Examine attentively a piece of glass on which gold has been melted, and the lateral tracks will, in every point, retain the violaceous purpureous colour; only, this colour will usually appear more languid, as the distance from the center of the stain will be greater, in consequence of the greater rarity of the particles; the smoky tracks from a silver stripe, will appear of a sky-blue colour, inclinable to green, which likewise gradually vanishes.

717. Even from the beginning, I melted a stripe of metal leaf, exposed to the open air, either placed upon a piece of glass, or suspended from the head of the conducting bow (*Art. Elect.* p. 134.) but the fusion was, in these cases, attended with a few circumstances different from those above, which could not but take place. Place a stripe of metal leaf upon a plate, with another plate cover only an half part of the stripe, and then send the spark through that part of the stripe which is pressed between the two pieces of glass, deep and lasting stains will arise from it;—from the other part of the stripe, a stain will result, which will be much more of the nature of smoke, and will easily be rubbed away by the hand. If the copiousness of the spark be increased, stains will take place, even in the open air, pretty dense and lasting.

718. By increasing the sparks, Mr. Kinnerfley brought them, from melting metallic leaves, to render red-hot, even in the open air, an iron wire the fifteenth part of an inch thick; the sparks could even drive it into sparkling drops, and destroy it. I attempted to repeat the experiment, but found the discharge of two coated plates insufficient.

719. I then attempted with the discharge of a single plate, to lengthen an iron wire, which was a sixteenth part of a line thick,

thick, or thereabout. In fig. 5. Pl. X. is represented the apparatus of which I made use for that purpose. I. Upon the horizontal table LM, the brass rule, AB, to which the dented wheel RM has been fitted, is so placed, as to describe an arch of a circle from the center A; to that end the brass rule is raised on two wheels or pulleys, QH, which move on a plate of brass well polished, and very even. II. The arch RM makes the small wheel B move, which carries the index BI. III. I insulate the whole on a glass plate, which I place on a coated sheet; I put a candlestick on the latter, in *e*, and then adjust the iron wire to the brass rule in D, and to the candlestick in Q. IV. Things being thus disposed, by presenting one end of the conducting bow to D, I make the spark rush through the wire DQ, eight inches long; and, in this very instant, the index BI runs along the arch *Ii*, which is about the twentieth part of the circle; the reason is, because when the wire DQ is lengthened by the spark, the spring EF becomes enabled to drive forwards the brass rule AC. Having more accurately calculated the quantity of the motion, I found it to be a ninth part of a line.

720. From this experiment, I also perceived how rapidly the small degree of heat in the wire, acts, and causes the brass rule to move, and how rapidly the same afterwards vanishes. When you watch with attention the motion of the index, in order to see it reach the point *i*, the shortest moment of absence is sufficient to make you lose sight of it, and you perceive it again, only when it returns to *u*: it is only in this last part of the arch which the index describes, that it moves with slowness and regularity.

721. A degree of heat more vehement, produces a lengthning of the wire, which is greater and more lasting. Mr. Kinnerley sent the discharge of thirty-six bottles through an iron wire two feet long, vertically drawn by a weight; the wire became red-hot, and was lengthened a full inch more than it was before; a second discharge not only lengthened the wire four inches, but broke it in the middle (Franklin, p. 392.) By means of the same battery, Mr. Kinnerley likewise melted an iron wire before Dr.

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Franklin, and so inflamed it, that drops fell from it, and for a while continued burning on the floor.

722. Mr. Canton and Dr. Priestley repeated afterwards this interesting experiment. Dr. Priestley, with his large batteries, drove iron wires into sparkling drops to the distance of several feet; he could even destroy them, though dipped in water, and the water was plentifully scattered away. Dr. Priestley observed, that the wires resisted more against being driven into drops, in proportion as their length was greater; that iron wires threw burning drops which sparkled more vividly and *amply*, than brass wires; that when the discharge was not very intense, several of the drops into which the metal had been scattered, assumed, when they cooled, the shape of small balls.

723. With a discharge of three large crystal jars, I can melt an iron wire, the sixteenth part of a line thick; and the stumps of it, which remain hanging from the two brass rods, continue to lighten, and throw sparkling drops during a sensible space of time afterwards: these drops are exactly like those produced by throwing iron filings through the flame of a burning candle. The small globules produced by an iron wire, when melted by a middling discharge, are like the blackish globules that are made to fall on a sheet of paper, by striking a piece of steel with a flint; they consist of iron nearly reduced to the state of scoriæ.

724. Nor is it necessary, in order to ascertain all the above facts, to make use of a very considerable battery; some few iron filings placed in a regular row, on the margin of the naked sheet of glass HQ (Pl. IV. fig. 9.) will, by means of a single discharge of the coated plate AB, exhibit sparkling drops, small globules, &c. I use the word to *melt*, because a more proper expression is wanting in order to express the above effect of electric sparks on metals; for, certainly, copper, tin, lead, and iron, are rather destroyed than melted, because their phlogistic is driven off, and they are turned into scoriæ, into calces, or even into glass; nor are the particles of gold, or silver wires, simply melted, this word only expressing a state of fluidity; they are scattered into small disjointed particles.

725. Hence

725. Hence we may derive a solution of the question proposed by Dr. Priestley (p. 729.) who could not find any reason why some metals are driven into drops, or vapours, by electric sparks, while others are only reduced to a state of fluidity. He sent discharges through pairs of wires of different metals, that were equally thick, equally long, and joined together; when he found, I. That the discharge completely dissipated an iron wire, and left a brass wire intire. II. That another discharge intirely dissipated another iron wire, and left a copper wire intire. III. That another discharge dissipated a copper wire, and left a brass wire intire; though, those wires which were preserved intire, were, the doctor remarks, somewhat melted in the place where they were in contact with the other wire. IV. That a certain degree of charge scattered a brass wire into vapour, and left a silver wire intire. V. That a certain quantity of charge destroyed a brass wire, and left a gold one intire. VI. That a certain degree of charge destroyed a silver wire, and left a gold one entire. From these observations, Dr. Priestley concludes, that metals may, with respect to their power of resisting the effects of the electric fire, be ranked in the following order: I. Iron; II. Copper; III. Brass; IV. Silver; V. Gold. Dr. Priestley adds another experiment, by which he proves, that lead is more easily destroyed than tin.

726. This power of metals of resisting sparks, follows a very regular order, even with respect to iron, which, though it is of itself the most difficultly melted of all metals, is more easily destroyed by electricity than copper, brass, &c. the reason is, because the effect of sparks is not so much to produce a complete fusion of the inward substance of metals (so that their phlogistic be preserved, and the metal only brought to a state of fluidity) as to drive off this phlogistic from the superficial parts of them; now, the facility of this dissipation of the phlogistic of metals, is not proportioned to the facility of their fusion, but to the facility with which their superficial parts may be deprived of this phlogistic; and it happens that the less perfect metals, when exposed to the action of common fire, lose their phlogistic, with a facility that exactly follows the order in which they respectively yield to the action of electric sparks. Lead may be turned into scorix more easily, and to a

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greater depth, than tin ; tin more easily, and to a greater depth than iron ; iron sooner, and to a greater depth than copper : the same observation may also be applied to the more perfect metals ; copper, when reduced to a state of fusion, loses more of its substance than silver ; and silver also suffers a greater loss than gold, which suffers none, or at most an exceedingly small one.

727. Hence may be explained the reason why sparks, when not very intense, only destroy the superficies of iron wires : the reason is the same as that for which common fire gives, according to its different degrees of intensity, different colours to iron, and at last brings it to the black dye of scorixæ ; a state into which it can more completely turn the superficial parts of it, than the internal ones, as the phlogistic of the latter is protected by the external parts. According to this principle, we see brads-founders, and especially iron-smiths, endeavour to protect the superficial parts of their metal, by keeping it covered with bodies extremely replete with phlogistic.

728. A more complete knowledge of the alterations produced both by common fire and electric fire in the substance of iron, and especially in its phlogistic, with which it seems remarkably to abound, would throw a great light on the connection between electricity and magnetism, and greatly contribute towards unveiling this important, though dark, mystery of nature.

729. It is a fact extremely well known, *that an electric spark of a certain degree of intensity, sent through a needle, gives it, takes from it, or even inverts in it, the magnetic direction.* This fact is more simple, and has a more extensive relation to our mundane system, than has been at first imagined, since *electric sparks can, in an instant, alter the magnetic direction of pieces of iron, in the same manner as particular positions of the same, can only effect after a long course of time.* So that it appears, that a strong spark does nothing more than introduce in an instant, the same alteration in the reciprocal situation of the particles of iron, as is slowly effected by the universal imperceptible current of the same fluid, which moves within our system with a given direction (*Atmos. Terr. Elect. pag. 260.*)

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730. The following is an account of the manner in which I discovered and ascertained the law, that electrical sparks introduce the magnetic direction into needles, according to the position in which they were placed when they received these sparks. If the spark, said I, turns the point of the needle into which it gets, towards the north, whatever may have been the situation of the needle, this is a particular effect produced by a particular action of the spark, which acts according to its own direction. This important point, in which the whole theory of electricity is concerned, and which surely is not indifferent to the science of magnetism, has so much attracted my attention, that I took myself to examine pieces of iron, ferrugineous stones, and bricks, which had been struck by lightning, and of which I had before observed the position, in order to form my conjectures with regard to the direction of lightning; and I saw that, when the direction of the stroke of lightning had been from north to south, or from south to north, all the above bodies attracted that point of the needle which verges towards the south, with some of their southern, or also of their northern parts; I, therefore, began to suspect that the magnetic direction introduced by lightning, depended not only on the direction of the latter, but also on the position of the bodies which had been struck*.

731. Then I passed to experiment with artificial sparks; and, in order to avoid the tedious task of setting needles afloat in a cup filled with water, as well as the danger of falling into errors, I got several needles to be made, as represented in Pl. X. fig. 6. to which a small *cap* made of crystal, was adapted; then I reasoned thus, if a spark introduces a magnetic direction into a needle, that depends on the position which the needle had when it was struck, no direction can be given to a needle placed at right angles with the magnetic meridian, that is, placed equally between a north and a south direction: in order to ascertain this, I used the discharges of two large jars of crystal. I. I struck a needle placed

* If the above paragraph, on the contents of which the author seems to lay a particular stress, had been more clearly expressed in the original, the translation of it would also have been somewhat clearer.

as above said, perpendicularly to the magnetic meridian, and sent the spark through the east point L; but how astonished was I, when having placed the needle on a bodkin, and covered it with a glass bell, it constantly assumed the same direction it had when I struck it! II. I placed the needle in the same position as before, and now introduced the discharge through the west point P; and having placed it again on the bodkin, it kept its former direction. III. I placed the needle on the same line as before, but inverted it; so that the point P now aimed at east, and the point L at west; and after sending a discharge, I found that the needle was now directed, the point P to east, and the point L to west. IV. Having again placed the needle in the same position as I had just now done, I now sent a discharge from west to east, and found that the needle still directed its point P to east, and its point L to west. I must confess, that it took me a considerable time before I could unravel the mystery; yet, the reason of it was the very same for the finding or ascertaining of which I had been first induced to make the experiment; that is to say, the spark by running through the needle, when placed at right angles with the meridian, gave a direction towards the north to that part of it, and of the *cap* in the middle of it, which lay on the north side, and gave a direction towards the south, to that part of the needle, and of the *cap*, which lay on the south side.

732. V. I placed the needle directly upon the meridian, and having sent the spark from north to south, I found that point of the needle directed towards the north, which had been placed towards the north. VI. I placed it again on the meridian, and having sent the discharge from south to north, I found that the needle was still directed in the same manner as before. VII. I again placed the needle on the meridian, but inverted it, and a single discharge, sent from south to north, inverted the direction of the needle, and the point which before verged to the north, now verged to the south. VIII. I again placed the needle in the inverted position just now mentioned, and another discharge, though now sent from north to south, confirmed the last direction of the needle.

733. Lastly, I placed the needle vertically; and, IX. A discharge sent through the upper point, caused the needle to direct its lower point to the north. X. I now introduced the discharge through the lower point, and the same direction was confirmed. I afterwards placed the needle in the same vertical position, but inverted, and a single discharge sent through the lower point, inverted also its direction. XII. Having again placed the needle in the latter position, and sent the discharge through the other, that is, through the upper point, still the latter direction was confirmed.

734. Hence we may universally conclude, that *sparks and lightning do not introduce a magnetic direction into bodies, that be determined by their own particular direction when they enter into those bodies, or get out of them*; in fact, that point of the needle which aimed at north when the stroke was sent, is always afterwards directed towards the north, whatever may have been the direction of the spark (whether northwards or southwards) in rushing through it; and (what is very singular, and may have been the cause of the mistakes of former observers) a spark sent with any direction whatever, through a needle placed at right angles with the meridian, always causes those parts of it to be directed to the north, which were placed on the north side when the stroke was sent. *But sparks and lightning introduce a magnetic direction into a needle, or other piece of iron, or into a ferrugineous body, according to the position which those bodies were in, when they received the stroke*; and, indeed, it is a fact very well known, that pieces of iron placed on the magnetic meridian, gradually acquire the property of directing towards the north, that part of them which aimed at north; and that, of pieces of iron vertically suspended, that part is afterwards directed to the north, which hung lowest when they received the stroke.

735. We must not, however, infer from thence, that the *polarity* of pieces of iron produced by a spark, or by lightning, is wholly to be attributed to their position; since, I. Lightning, as we shall see hereafter, may create a far greater magnetical power of supporting weights, than any which may arise from the position alone of bodies. II. No brick, no ferrugineous stone, can:

can acquire from its position alone, the power of attracting a magnetic needle, but may receive it from a stroke of lightning.

III. The position alone, as I mentioned above, does not give so quickly a polarity to a piece of iron, as a spark can do, nor does it give it to smaller needles, in so high a degree.

736. Are not these peculiar effects of the electric fire with respect to magnetism, so many proofs which corroborate my former conjectures, that the peculiar magnetic force observed in *load-stone*, is to be attributed to either atmospherical, or subterraneous, strokes of lightning; and that the *universal systematical* properties of magnetic bodies, are produced by an universal systematical circulation of the electric element? It is true, magnetism is excited by other means than electricity, such as heating, hammering, rubbing, breaking, &c. but is not common fire or heat, also excited in a number of different ways; and yet, it is always found to be the principal agent, and, as it were, the author of its own phenomena.

727. But of these conjectures, I shall treat more at large in *Nat. Elect.* now, proceeding farther on the works of Art as to electricity and magnetism, I declare, that I never could by any method whatever, introduce any sign of direction, either towards the pole, or towards similar metals, in needles made of gold, of silver, or of copper. I do not mention this in order to confute the opinion of those who, on that subject, quote the authority of Aldovrandi in his *Metallurgia*, which indeed in these days, would be needless; but only, in order to observe, that the property of *magnetism* is wholly peculiar to iron; this metal is remarkably replete with inflammable substance, and whenever a too intense spark drives this substance from it, it instantly loses every disposition to acquire magnetic properties; which circumstance greatly increases my suspicion, that some igneous principle is the cause of such properties, and that this principle produces them in bodies, by modifying, after some peculiar manner, the inflammable substance which they contain.

738. But what other igneous substance is more universal, either with respect to its diffusion, or its operations, than the electric fire? Of its diffusion, I have treated in the first section of
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of this work; I shall now relate the different experiments with which, in the *Atmos. Terr. Elect.* (p. 254.) I found means to vitrify, to calcinate, or to metallify, several bodies, conformably to their nature; and thus shewed, that the electric fire produces in an instant, the same alterations in bodies, which common fire only can effect gradually and slowly. I. I reduced the substances whether metallic, or of other kind, on which I intended to experiment, into powder, which I carefully dried. II. I put such a quantity of it in a small tube, half a line wide, as might form a small cylinder half a line high, or more, according as the substance I used was more or less insulating. III. I rammed this cylinder, within the tube, with two small iron rods. IV. And lastly, I wrapped the tube with strong paper, that I might afterwards examine the pieces into which the tube was often shattered, especially when I experimented on substances, from which very elastic vapours arose. V. The spark was usually formed by the discharge of two coated plates, the sum of the coatings of which was about seven square feet.

739. I. Powder of Borax, thus inclosed in a small glass tube, appeared at first white and opacous; after being traversed by a spark, it appeared like a solid body, which was transparent, and adhered to the glass; the latter circumstance was owing to the spark having melted also the inner superficies of the glass, as might be discovered with a lens, or even without such assistance (*Atmos. Terr. Elect.* p. 254.) II. Scrapings from *laminous* jet became calcinated; the few parts of them through which the spark had passed, could imbibe water, and were thereby turned into a kind of mud. III. Litharge of lead acquired a yellowish transparent colour, like that of amber, which is the colour of litharge when vitrified. IV. From verdegrease there constantly resulted a spot of a vivid deep red colour, and as glossy as polished marble, which I immediately acknowledged to be *glass* of copper. V. From calx of antimony, I obtained a spot of such a yellow colour, and such a transparency, as made me acknowledge it to be *glass* of antimony. VI. A little powder of zinc, at the instant that it was traversed by the spark, threw two small flashes of whitish smoke, out of both ends of the tube, which furred so much the inner

inner surface of it, that *flowers* of zinc were very easily discernible on it.

740. With respect to *metallifying* again metallic *calces*, it appears, that sparks do not effect it otherwise than by driving into the lateral surfaces of those *calces*, what remaining phlogistic they may find in those parts of them, which they immediately enter. It is true, that the smoky track effected by the passage of a spark through a bit of cinnabar, is all over sprinkled with drops of mercury, and I have seen such drops also arise from several other mercurial compounds; but these drops, we are to observe, were not *revivified*, they were merely made to unite again together; for the mercury, in all the above compounds, retains every requisite substance or principle to make it exist as mercury, only they are divided into such small particles as render them unperceivable to our senses. I have repeated several of the experiments above, by sending the discharges of three large crystal jars through the several substances above mentioned, after inclosing them between cakes of wax; and the effects were exactly the same with those before, only somewhat more intense.

741. The following experiment may, I think, serve to shew how sparks can turn metallic bodies into real metal, by driving and carrying to them such phlogistic as they may raise from the places of their passages, and imprinting it into the surface of those metallic substances. With a little oil, anoint some small spot on the surface of a well polished plate of iron, and send a spark through that spot, you shall find its colour turned into an almost argentine colour, like that of the finest steel; which, I think, is owing to the phlogistic which has been added to the iron. The same will obtain if wax, or tallow, are used instead of oil; without some such like ointment, sparks can produce no such effects.

C H A P. V.

On the electric fire, relative to common fire.

742. **I**N examining all the above effects of electric sparks, with respect to air, to water, to living bodies, and to fossils, we may perceive that they are similar to those produced by common fire; and all the difference between them may be reduced to the following, viz. that common fire acts with great slowness, when compared with the electric fire, and with less efficacy; which, perhaps, is owing both to its being less dense than the former, and being mixed with inactive substances.

743. This similarity is rendered still more obvious, by considering how easily either spirit of wine, the smoke of a candle just put out, powder of colophony, or gunpowder, and such like inflammable bodies, may be kindled even by very small sparks, in consequence of their phlogistic being very copious, and little fettered by either aqueous, saline, or terrestrial particles. The least imaginable spark will inflame æther; ordinary sparks sent through a fur of rectified spirits of wine, a little warmed if the experiment be made in winter, will kindle it; even sparks drawn from a piece of ice will light a candle just extinguished. A strong spark from the Chain merely, will kindle the smoke of colophony; but, in order to inflame bare colophony reduced into powder, the strongest spark from a coated plate must be sent through it; to that end, the powder must be spread on the coating of the plate, and a little warmed if the experiment be made in winter, and the sparks must be drawn through this powder; with regard to gunpowder, it is necessary, in order to kindle it, to ram in and carefully compress its particles, in order the more to condense the spark between them.

744. With regard to other bodies less inflammable than those above, there is this analogy between them, that the electric fire kindles pretty easily those in which it finds a sufficient quantity of phlogistic, or which stand near enough to bodies replete with

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that substance, to enable the electric fire to drive a certain quantity of it into them. Place a leaf of paper, anointed with oil, wax, or tallow, in the middle between two quires of paper, and under the whole, place a sheet of tinfoil; then send a large spark, such as that from my three large crystal jars, and you shall find that both the anointed leaf of paper, and the two next, have been burnt around the hole made in them by the spark; you shall even find the edge of the hole in the last leaf, which is contiguous to the sheet of tinfoil, blackened a little; and if you examine this latter hole with a lens, you shall find it almost filled with particles of melted tin; with regard to the leaves in the middle of the quire, they preserve their usual colour. It appears that the rapid spark can disengage a certain quantity of phlogistic, only where it finds it in a certain degree of copiousness; it seems also that the spark avails itself much of the phlogistic it has thus exploded, for kindling such bodies as stand near, though less replete with it; but of all this I shall speak more fully, when I come to treat of lightning, as it will afford us much more extensive and expressive examples. At the *Superga*, the silk strings which keep the wires separated, are often singed by the burning flashes of phlogistic, which the strong sparks that are thrown from the neighbouring extremities of these wires drive against them.

745. Another fine analogy between the electric and the common fire, I also discover in the rings of prismatic colours which they equally produce, and which Dr. Priestley only considers as particular effects of electric sparks. The doctor has described these rings in the fifty-eighth volume of the *Phil. Trans.* I receive only now that volume, with the following (the fifty-ninth), and, along with them, the Memoir concerning the above *rings*. I find two others, besides the above; the one treats of the lateral strength of electric explosions, and the substance of it is the same with what I said in p. 77 of *Atmos. Terr. Elect.* with respect to the lateral explosion of air. In the other Memoir, I find that Dr. Priestley had discovered, before me, the peculiar resistance which a discharge meets, when obliged to circulate through a long space, within deferent substances. It was a fine experiment which he made, by using a pair of tongs, instead of a conducting

ducting bow, for discharging a plate of glass: when the two branches of the tongs had been brought pretty near to each other, he observed, that a part of the discharge, instead of running through the long circuit of those two branches, preferred to leap from the one to the other, through the air.

746. The substance of the other Memoir, of which I am now more particularly to speak, may be reduced to this point, viz. that a strong spark from three glass jars, which is made to rush from a plate of metal into the point of a needle perpendicularly placed upon it, leaves around that part of the plate through which it passes, and which it deeply melts, several very thin concentric circular rings, imprinted on the metals, which are like the colours of the rainbow. From this phenomenon I derive another analogy between the electric and the common fire. I light a few small coals on a forge, exactly under the funnel; upon these charcoals I place a thin, long, and polished spring of steel, which is folded; then examining it attentively around the center of the action of the heat, I see the same colours take place as those above; only, the bands which form such appearances, are the more tortuous and broad, in proportion as the heat of the coals proceeds in the present case from a greater number of parts disposed with less regularity.

747. Universally, the heat produced by electric sparks, or by common fire, extends over a blade of steel, with an intensity which lessens, in proportion as the distance from the principle of such heat increases; and these two species of heat thus reduce the superficial parts of the blade to the respective degrees of tenuity which are requisite, that they may, according to the calculation of the great Sir Isaac Newton, reflect rays of various colours; we are, therefore, by these experiments brought acquainted with the existence of a simple geometrical regularity obtaining in the operations of heat, in which our senses had hitherto allowed us only to perceive a kind of perturbed motion.

748. Having placed a thin, polished, ample plate of brass, and another of silver, on charcoals, I saw on them the same series of colours as above. I. The experiment of Dr. Priestley, who, by repeatedly sending fresh sparks, widened the internal rings

of colours, is not inconsistent with the explanation above given; new sparks bring more distant particles of the plate to a state of greater tenuity than before, and the effect is in this case the same as that which is so remarkable in bubbles made with soap-water, the upper part of which grows continually thinner, in consequence of the weight of the water, and the coloured circles formed by it become gradually broader. II. The circumstance of the rings being rendered ampler when the point of the needle remains a little distant from the plate, is consistent with the other effects of sparks; as these sparks leap farther from the point, they must needs spread themselves more, in the same manner as electric brushes do.

749. But of all analogies between the electric and common fires, the most remarkable is that of the manner in which they propagate themselves through different mediums, or bodies; in regard to this, the following rule obtains, viz. *the common fire propagates itself with most difficulty, through such bodies as refuse to conduct the electric fire, and most easily through those which can conduct it.* Dr. Franklin (pag. 259.) first mentioned this analogy in a letter which he sent to the Royal Society, in the year 1756; I shall here bestow a few words on the same subject. I. *The common as well as the electric fire, are speedily dissipated in dilated air.* The following experiment will serve to shew the truth both of this fact, and of the reason assigned in 277, why a flame is quickly extinguished in dilated air. I hold a round spoon, an inch and an half wide, under the beak of a lamp, and having lighted some paper placed upon it, the flame of the lamp is extinguished; the reason of this is, that the ample flame of the paper drives the air from the space that surrounds the flame of the lamp, whence the whole fire of the latter is invisibly dissipated, and forsakes the wick. The spontaneous flashes of fire emitted by the phosphorus of Kunkel under the glass bell of an air pump, which is gradually consumed, proportionally to the copiousness of these flashes, is very conformable to the explanation just given; the facility with which the copious fire contained in this phosphorus can of itself escape (which makes it necessary to keep it constantly dipped in water), enables it still more easily to fly and burn away, when placed

placed in a vacuum; now, the flame of a burning candle is dissipated exactly in the same manner; only, the greater adhesion of the fire to the substance, which is the pabulum of this flame, is the cause that one flash being once dissipated, another does not succeed.

751. *Common fire, and common heat, are, as well as electric fire, retained proportionally longer, in denser air.* A certain quantity of air laterally driven against a flame, separates it from its pabulum, so that it can excite no new fire, or flame, from it: but when air is driven directly against the ignited body, it carries the fire which is disengaged out of the same, against the aqueous, saline, or terrestrial particles of which it is composed; it thereby again disengages the fire which lies inclosed within them, and thus animates the flame. If we raise our thoughts to more extensive contemplations, we shall be likewise convinced of the infinite goodness and wisdom of Providence, that has immersed us in a medium like air, which so efficaciously repercusses and preserves our vital heat. Is it not another effect of the same infinite wisdom, that heat being only preserved in a small degree on the tops of mountains, in consequence of the rarity of air, rains fall there not only with great plenty, but also concentered under the shape of snow; so that they become, as it were, coacervated to a great height, and, flowing from thence with slowness, perpetually maintain the reservoirs which form rivers, that is to say, the springs of the fertility, as well as the industry, of the populous plains.

752. II. *Common fire like the electric fire is easily dissipated through moist vapours.* The only reason, I think, why a candle placed above the surface of boiling water, or of grapes, which are strongly fermenting, soon goes out, is because that portion of fire which is already disengaged from the pabulum, can then freely escape through the moist vapour raised from the grapes, or from the water; it was by vapours of this kind, plentifully diffused in an open space, and continually succeeding each other, that the flame in numb. 277, was extinguished. If we proceed to experiment on permanent inclosed vapours, we shall find, that the analogy between electric and common fire still continues to obtain; nay, it proceeds farther, and likewise takes place in phenomena of a contrary

contrary kind to those above mentioned; that is to say, *as those bodies which can conduct the electric fire, yet refuse to transmit more than a certain quantity of it through their own substance, so inclosed permanent vapours, impregnated with a certain quantity of common fire, which they cannot dissipate by transmitting it to other bodies, will suffocate a flame.* To prove this, you may use the simple experiment of an extinguisher of paper, made by properly fixing a piece of paper to the ring of a large key. Put this extinguisher over the flame of a candle, and as you lower it, the flame will shorten itself, without penetrating into the paper; indeed the latter will be half burnt and blackened by the hot effluvium, but will not be kindled. The flame cannot diffuse itself away, through the vapour contained in the extinguisher, as it would do in the open air; and this vapour being once replete with fire, suffocates the flame, by refusing and repercussing it: to be short, the vapour inclosed and confined in the extinguisher, confines the flame, in the same manner as the particles of this same vapour, before their being disengaged from the pabulum, imprisoned the fire in it, and fixed it.

753. I prick the extinguisher with a needle, when a smoke, but no flame, rushes out of it: this is because the vapour which continues to be stopped, still possesses a greater efficiency for suffocating the flame, than for animating the small part of it that is now allowed to depart: this is likewise the reason for which, from too small intervals between pieces of burning wood, no flame, but only smoke, can spring out. Out of these suffocated inclosed vapours, that from gunpowder must be excepted; the strength with which its flame propagates itself, which is owing to the affinity between charcoal and nitre, exceeds, at least to a certain degree, the suffocating force of the vapour, and the latter, which spreads itself as usual, with a very great expansive force, spreads itself (owing to the said affinity) actually kindled and inflamed.

754. Let us now return to our subject. In order to prove the common property of electrical and common fire, of being dissipated by moist vapours, this observation surely suffices, that all bodies, in general grow cool, in proportion as a greater quantity of moisture emanates from them. Whence it happens, that by wetting
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a thermometer exposed to a current of air, with any kind of liquor, the mercury in it lowers in proportion to the quantity of moisture which evaporates from it. Thence also the experiment made by Dr. Franklin, who, wetting the ball of a thermometer with æther, while another person blew against it with a pair of bellows, saw the mercury in it lowered several degrees under the degree of congelation. In summer, I place a stratum of ice six or eight lines thick, on a table which is unpolished. I leave it till it begins to melt, then I cover the upper surface of it with salt, and while this upper surface melts and gives a smoke, the under surface, which at first thawed, now begins to freeze again, and sticks to the table. This is to be explained by considering, that the vapour emitted from the upper surface of the ice, serves to carry the remaining heat of the under surface, in the same manner as the vapour which rose from the ball of the thermometer above mentioned, dissipates the heat in it. Thence proceeds the coolness of water contained in vessels, either wrapped in wetted cloths, or made of clay which has not been varnished, and through which the water continually oozes. Whence the coolness of the leaves of trees, if not from the moisture which continually perspires from them? Whence the coolness of meadows, if not from the continually emanating vapours, which, besides the heat of the grass itself, carry away successive portions of that which lies diffused in the ambient air.

755. Our vital function consists, in a great measure, in disclosing fire from our aliments; which is effected by triturating them, by mixing them together, by making them circulate, and by diffusing into them the heat already diffused in former juices, in order to disclose that which they contain. It is necessary that this heat, which perpetually reproduces, or excites itself, in the same manner as the flame of a candle does, should perpetually emanate from our bodies, in a certain quantity, exactly as the flame of the candle requires, in order to its continuing to live, to be continually diffused away; now aqueous liquids are, in animal bodies, the chief conductors of this heat. Thus it is, that harvest-men preserve themselves from the deadly effects of the external heat, which would otherwise keep within them their own internal heat, and increase

crease it to too high a degree, by plentifully drinking and perspiring; thus, we can by the help of a fan, cool ourselves, by bringing continually new dry air into contact with our face, which may carry away moist vapours, and, consequently, the heat from it. The seaman, says Dr. Franklin, in his letter XXVI. in which this great Observer treats this subject with his wonted sagacity, the seaman puts his finger into his mouth, and from that part of it which he feels to grow cool soonest, judges from whence the wind is to blow. If such emanation of vapour from our bodies be too copious, then our vital flame is too much dissipated; it is thus we feel a painful coldness, when we get out of water after bathing; the moist fur which covers our body, by being dissipated, carries away too much of that heat, which I may call *innate* with us, since we bring it into the world from the maternal womb, along with those vital functions, by the help of which we afterwards preserve it.

756. Cannot we, in the same manner, impute all kind of artificial cold, especially those produced by chemical mixtures, to a dissipation of heat, caused by a dissipation of some vapour, in those mixtures, which is apt to conduct common fire away with great efficiency? This sagacious extensive conjecture also belongs to Dr. Franklin; and, besides, agrees with the method described by Mr. Muschenbroeck, in his Commentaries on the academy *del Cimento*, for lowering the mercury of a thermometer; which is by dipping it into a number of flakes of snow, which are successively destroyed by means of fuming spirit of nitre. The latter is the method by which they have, at Petersburg, succeeded to coagulate mercury; though, I must add, that I think that as Nature hath two ways of cooling bodies (the one by dissipating their fire, the other by fixing it) so, we must allow to Art also, two different ways of producing cold. When the acid of vitriol, which is disclosed by rust, from a ferrugineous body, is mixed with the phlogistic of powdered charcoal, only an external transient heat is manifested, but the greater part of the fire of this powdered charcoal remains fixed, as I think, in the small mass of *sulphur*, which results from the mixture.

757. III. *Does not common fire propagate itself more easily through metallic bodies, than through other such bodies as repress or refuse electrical*

electrical fire? This is evident, from the much greater facility with which water boils in vessels of iron, of brass, or of tin, than in those made of varnished clay; which observation may serve to throw a light on the useful art of keeping heat more united, more intense, and preserving it longer, &c. Dr. Franklin poured melted lead into a kind of square box, the bottom, and three sides of which, were of wood, and the other side of lead; and, he observed, that the latter side was sooner heated through, and caused a quicker coagulation of the melted metal than the others.

758. This analogy may be carried much farther; *heat may render such bodies conductors of the electrical fire, as would otherwise refuse to conduct it; and cannot we also think, that the quantity of common heat already introduced into a body, enables it to receive and transfuse a still greater quantity of it?* Thus, the heat of a lighted candle, by gradually penetrating into the wick, excites another effluvium from it, which is ready to kindle, and fly away in a vertical direction. It is the heat of the smoke arising from a candle just put out, that causes it, when received in a tube of paper eight inches long, or more, to kindle, as soon as a lighted candle is brought near the upper orifice of the tube; when the flame of the latter seems to precipitate itself towards the wick of the former.

759. *Is not, moreover, the property of electrical fire of disposing deferent vapours into its way, likewise proper to common fire,* besides their noted common propriety, of always *diffusing themselves to an equality?* It is, no doubt, the common fire, that, by diffusing itself to an equality from the ambient air towards those bottles or jars which are brought cool from the cellar, carries to them the deferent moisture which we find to coat them. It is also common fire, which, in winter, by diffusing itself from a warm room, towards the cold panes of glass in the window, carries to them the deferent moisture, which, by losing its fire close to them, begins to freeze. It is the subterraneous heat which, either the rivers, or the sea, continually draw from their beds, which diffuses temperate deferent mists into the ambient air. The following is a fact which I constantly find to take place: whenever, in winter time, the mist on the river Po, which is distant only one thousand four hundred paces from

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my habitation, reaches to the height of a certain window on the north side of my apartment, the mercury of the thermometer which I keep at that window, is suddenly lowered one or two degrees; which is owing to the heat which diffuses itself from the river, and carries this flow mist along with it; in the same manner as, in a cold morning, the excess of the subterraneous heat raises a very conspicuous deferent vapour from the bottom of wells.

C H A P. IV.

On electric sparks relative to light.

760. **C**OMMON fire, when displayed over the surface of inflammable bodies, assumes colours which are extremely varied, according to the nature, whether aqueous, terrestrial, or saline, of the substances by which it was imprisoned, and with the effluvia of which it always, though being in an active state, remains more or less mixed. As to electrical fire, it is so much less adherent than common fire to the substance of bodies, that it commonly manifests itself free from any mixture of them; nor does it seem that it exhibits any other difference in its appearances, than what may result from its different degrees of density.

761. *However, ought not the deferent vapours that conduct intense sparks along with them, produce some change in the appearance of the latter?* Indeed, to this cause, I think, we are to impute, at least in some degree, the remarkably red colour, assumed by the spark which Dr. Priestley sent along the surface of a vessel full of oil of vitriol. The reddish colour of sparks, sent into animal bodies, is, very probably, likewise owing to the vapour which they raise, either from feathers, or from hair; the singeing of the latter, as well as the smell arising from it, furnishes a sufficient proof of the existence of such vapours. The spark which imprinted metallic particles, into the above mentioned cylinder of glass (700) seems to have been loaded with a great quantity of such particles. Now, must not the outward appearance of this spark have been greatly

greatly altered, in consequence of the strange substances which, as it were, vested it? indeed, accounts are very frequent of lightning that has been propagated almost invisibly, through extensive tracts of very thick clouds.

762. If we except the cases in which the appearance of the light of the electric fire, is altered by such accidents as those above mentioned, we may safely say, that this fire is of itself extremely pure; and I never could understand how it might come into the head of some philosophers, to think that the light of electric sparks does not contain the seven prismatic colours. Do we not see objects coloured and disposed in the same manner, by the light of electric sparks, as by that of a candle, or of the sun? and if any doubt on that subject remained, the inspection of such sparks through a prism, would soon remove it. The first Observers who thought of making an experiment on that subject, did not perhaps take care to direct their eye towards that place where the spark appears to be, when looked at through the prism, which is very different from the place where it really passes; and others have, perhaps, made haste to copy the observation, merely because it was an extraordinary one.

763. The light of electric sparks, like every other kind of light, must naturally have the property of raising a *phosphoreity*, that is a power of giving light in the dark, on the surface of several bodies; yet it is only chance that has discovered this property to M. de Lane, so flow our reasonings sometimes are. Dr. Franklin gave me the first information of this discovery; and, at the same time, sent me some of the excellent phosphorus discovered by Mr. Canton, informing me, that it continued to emit light for a considerable time, when the flash of a spark had been excited near it. I thereupon betook myself to make experiments on the numerous class of phosphoreous bodies, enumerated by my respected friend, Signor Beccari, in the *Comentari Bolognesi*, and was soon enabled to answer Dr. Franklin, that the *phosphoreity* of the electric light was the same with that of the solar light. I shall relate here some of the experiments of which I wrote him the description. A full discharge of my usual coated plate, sent through powdered sugar, produced the beautiful light of a copious luminous shower, and

left, as it were, a *glory** of light around the place which drew the sparks. I also found that a like *glory* remained imprinted on the surface of a very fine dry linen cloth. Those bodies which Signor Beccari has observed to retain solar light in the highest degree, I have observed likewise to retain best the light of electric sparks.

764. I made also, in this same letter, if I remember right, a digression on the light emitted by salts while they are pounding. Sugar, for instance, while it is pounding, emits light more easily than any other salt, as well as more plentifully, and more like that of electric sparks. In order to observe the light yielded by gem salt, rock-alum, and by other salts in general, the eye must be well prepared, and intirely free, to use a phrase agreeable to the subject, from any phosphoreity of its own; and such salts must, besides, be pounded with blows that rapidly succeed each other. You may, when in the dark, frighten simple people only by chewing lumps of sugar, and, in the meanwhile, keeping your mouth open; which will appear to them as if full of fire: to this add, that the light from sugar is the more copious in proportion as the sugar is purer. By breaking in the dark the outward crust of extremely refined sugar, which covers certain comfits, a light springs out of it, which is exactly like that of electric sparks. I must, however, confess, that notwithstanding the similitude of the outward accidents, I could not succeed to ascertain whether the substance which lightened, while salts were pounding or breaking is the same with the light of the electric fire: very thin threads applied both within and without, to the sides of a glass mortar in which I pounded refined sugar with a glass pestle, produced no sign whatever of electricity; neither did threads placed close to the sugar itself, or affixed to the insulated tongs with which I ground it, afford any electric sign.

* A *glory*, is a word used in Catholic countries, to express that luminous splendid appearance, which is supposed to surround the heads of saints, of the Virgin Mary, or of God himself, and with which they are represented in the pictures that are made of them; hence the metaphorical expression, a *glory* of light, has been formed, which is frequently used in common speech and writing, and is meant to signify any kind of extensive luminous appearance.

765. This light yielded by salts must, therefore, proceed from a substance which finds itself in much the same state within them, as common fire is within bodies in general; and the violent separation of the particles of those salts, vibrates such substance through very short intervals, and without intirely freeing it out of the pores in which it is contained. But are not salts the chief links used by nature for confining and fixing the substance of fire? What are pyrites in the mineral kingdom, but salts, which, being united with a few metallic particles, confine a great quantity of fire? and in the vegetable kingdom, does not common fire disengage itself the more easily from woods, in proportion as the latter are more free from saline particles; whether they are naturally so, or a dissolving moisture has rendered them such. Why is the flame which rises from beech-wood that has been sent down rivers in floats, so mild and clear, if it is not because water has dissolved the salts which the wood contained? Why does wood decayed by lying during a long time in moisture, emit light; and why is it, at the same time, much lighter and brittler than common wood, if it is not because such moisture has dissolved and carried away those heavy tenacious links, salts? In consequence of this dissolution, fire is become enabled to disengage itself by virtue of its expansive force, from its now widened cells, and to begin to move, though indeed with such slowness, and in such small quantity, as only allow it to yield light, but not to afford any heat that may be perceived by our senses. Is not the above, the true reason of the light produced by cockle-fish and sea-fish, and of that which appears, in dark nights, on the surface of the sea, and is caused, as Mr. Canton has shewn, by floating substances that putrefy.

766. One thing I have succeeded to ascertain with respect to another kind of light, which, as far as I know, has not been observed by other experiment-makers, and is, as I have found, a real electric light. *I mean the light emitted by glass, and other insulating bodies of a certain thinness, when violently struck by air.* Here follows the substance of what, on a former occasion, I wrote with regard to the discovery and nature of such light, to my illustrious friend the Count of Scarnafigi.

767. I.

767. I. Wishing to discover something with respect to the electric fire, with which the substance of our air is replete, I had formerly attempted to see whether, while I drew the air from a glass bell, in the dark, I could perceive any light, or whether I could perceive any motion in the hair-threads which I had fixed to the said bell; but my endeavours proved ineffectual.

768. II. Suspecting that the small electricity, which I supposed to be produced by the dilatation of the air, might remain unperceived, on account of the slowness of this dilatation, and imagining no possible means of suddenly producing a vacuum, I thought of breaking in the dark a few glass balls that had been blown, when I saw an instantaneous light take place, the vividness and ampleness of which was proportioned, it seemed, to the ampleness of the ball, and likewise to the noise which the air made, when suddenly filling the vacuum that was opened to it. This light was much like the mild light which is diffused by those flashes of lightning which take place in stormy weather, and extend beyond the limits of the clouds, without offending the eye.

769. III. In breaking these balls, a smoky smell takes place, which most probably proceeds from the green rushes which are put in those balls, when they are made, in order that the vapour from them may swell the balls. Desiring to ascertain whether the light which appeared when the balls were broken, did not also proceed from a certain quantity of fire that was disengaged from this same smoke, I procured balls blown with a lamp, and they also emitted light, in a less degree indeed than the former, but very visibly, especially when I broke them within a large cup of white clay, and took care not to shut my eyes at the instant that I struck the ball. The decrease of the intensity of the light in the latter case, was proportioned to the diminution of the exactness of the vacuum, and of the bulk of the balls; these two circumstances caused the explosion to be effected with much less force in the latter, than in the former instances.

770. In order to ascertain still farther the truth of the above reasoning, I narrowed, with the help of a lamp, the neck of
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some Florence flasks; I introduced some water into them, which I made boil, and then suddenly closed their orifices; this done, I found, when I broke them, that they emitted in the dark as much light at least as the balls that had been blown at the furnace; therefore, the light of these balls does not arise from any smoke contained in them, and the cause above assigned to the diminution of the light that took place when only balls made with a lamp were used, was the true one.

771. In order still more to free the above light from any suspicion of a mixture of common fire, I drew the air from several large flasks with an air-pump, and they all gave me a light when I broke them; which light was proportioned, as to its extent and vividity, to the bigness of the flask, and the loudness of the explosion that had taken place. This proportion I have found pretty constant in all my experiments. Such balls as were cracked and somewhat full of air when I broke them, afforded no light, as well as no noise; others which received only a crack from their fall, imbibed air slowly, and did not lighten; other balls, or flasks, only imperfectly emptied of air at first, produced both a small light, and a small noise.

772. Care must be had, when larger flasks are broken, lest shivers from them should leap to the face, and endanger the eyes of the observer. When I experiment with those flasks, I use to look through a plate of crystal; and in order to preserve it from being obscured by my breathing, I adapt to my nose and mouth a sheet of paper which protects it.

773. In order to discover whether the light I mention proceeded from the stroke of air, or whether it simply arose from the breaking of the glass, I took to experiment with a few of the celebrated pieces of glass, sometimes called *Batavic drops*, which, as I had observed before, equally break in the vacuum, and in air; and by their means I procured instances of impetuous fractures of glass, made without any stroke from air. I therefore broke a large quantity of them, and neither I, nor any other person, ever could perceive in the completest darkness we could produce, any light arising from them. M. Poliniere says, he once saw a small light, which begun at the place of the breaking, and ran up to the head of the drop.

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The Abbé Nollet says, that he thinks he remembers his having once seen some such light; but, to me, it appears, that M. Poliniere saw too much, and that the Abbé was satisfied with too little.

774. In those Batavic drops, however, a few small bubbles are to be discerned: these gave me some mistrust, lest they should be filled by air, and I tried again the experiment upon *Bologna bottles*, the thick bottom of which was without any such bubble; their shivers, when they broke, were driven away with great force, but I could not perceive the least light from them. One indeed of these bottles emitted a light, but this circumstance was joined with that of a pretty loud crack, and the bottle was, as I found, both a Bologna bottle, and a *ball* emptied of air at the same time, like those above mentioned: I had hermetically sealed it at the furnace, and setting apart the thickness of its bottom, it was much like those flasks into which, a few years before, I had seen the noble and sagacious Count of Scarnafigi, put shavings of tin, which he afterwards kept for a long time in a state of fusion, with the flame of a lamp of spirit of wine, in order to show that they received thence no increase of their former weight.

775. I broke again several Batavic drops, in the vacuum of the air pump, and could not perceive the least light.

776. Having thus ascertained, as far as I possibly could, that the light perceived in breaking glass *bombs* and flasks, arose only from the stroke of the air, I undertook to examine whether such a stroke against any other substance would produce the like effect. I, at several times tied bladders on the orifice of a thick drum of crystal, which I used as a glass bell, and when the outward column of air broke through the bladder, and precipitated itself through it, I could not perceive any light. I very often by using thick strong bladders, succeeded to bring the vacuum within such drums to a great degree of exactness, and at other times, I broke the bladder with my finger, when only ten, eight, five, or only four strokes of the embolus had been given, and I never could perceive the least appearance of light; whence I concluded, that the light perceived when glass bombs and flasks were broke

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did not proceed from the stroke of the outward air against a remnant of inward air.

777. I now placed under the drum a very thin flask of glass, and when the air suddenly broke through the bladder skin, I had the pleasure to see small flashes of lightning arise in the place where I knew the flask lay. I afterwards substituted flasks of thick glass, and the stroke of the air against them did not produce any light. I also placed several vessels, or plates, of different metals, and of different sizes and shapes, within the drum, and no light ever resulted from the stroke. I afterwards placed under the drum a cylindric vessel of sealing-wax, an inch wide, three inches high, and pretty thin, and at every time that I made the experiment, I saw it lighten, especially around the brim, which was thinner than the body of it, and somewhat uneven; this circumstance I shall add, that none of the strokes of air, though ever so vehement, ever happened to break the vessel; which was owing, I think, to the equality and simultaneousness of the stroke, on both the internal and external parts of it.

778. Thus, through a series of experiments, I ascertained this small truth, viz. *that the light emitted by glass bombs and flasks emptied of air, is an electrical light, which the impetuous stroke, or vehement friction of the air, excites from insulating bodies*; this particular fact does not indeed discover any new relation between light and the electric fire; but it affords a new proof of the fact generally observed, viz. that the electric fire emits light whenever it is made to flow between insulating bodies, along their surfaces.

779. It might be here the proper place to examine this interesting question, how it happens, on the one hand, that electric fire cannot pass through insulating substances, and on the other, that *electric* light so freely pervades them. In fact, the spark which is thrown in the inside of a glass vessel, is seen from all sides: even through common wax, or sealing-wax, you may see the sparks excited on their other surface. In the first chapters of this work, I considered this fact as very singular; though, in truth, it is only analogous to what likewise happens in the above mentioned substances, with respect to common light. If you look

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underneath the edge of the top of a lighted candle, whether it be of wax or of tallow, you shall see it to be penetrated by the light of the flame. Adjust strata of the same substances to a hole made in a well closed window, and which is the only passage for light into the room, and you shall see the room lighted to a certain degree, by the light that will penetrate through the above substances. In order to explain these facts, the common hypothesis might suffice, that light propagates itself through the pressure of a medium which is universally diffused; and in this case we might likewise conceive this medium as being also vibrated within insulating substances. But to this hypothesis, the considerations offered by the sagacious Sir Isaac Newton, as well as the experiments made by Mr. Canton, are contrary. The latter gentleman has, in the Philosophical Transactions, in the treatise in which he gives a receipt for easily making excellent phosphorus of Bologna, has, I say, shewn how to make such phosphorus preserve for ever its virtue; which is by hermetically sealing it within glass very carefully dried; and he proves, that this phosphorus, partly of itself, partly by being heated, gives back what light it has received, and no more. To the above hypothesis are also contrary the experiments I have made, which I mentioned last year to Mr. Canton, and have more fully described to Father Boscovich: by these experiments I proved farther, that the same phosphorus gives back the light it has imbibed, just such as it received it; that is, of a same hue as that of the different crystals through which it received it. We may, therefore say, in order to explain the question proposed above, that the light emitted by an electric spark, consists of particles incredibly more subtle than the spark itself, and that the quantity of its substance is almost nothing, when compared to that which forms the body of the spark. The better light enables us to perceive other material substances, the less our senses are able to penetrate into the substance of light itself; and indeed it seems that the analogy fails between its *materiality*, and the *materiality* of other substances.

S E C T I O N V.

On the electric tickling and wind; on the brush and the star.

C H A P. I.

On the electric tickling and wind.

780. **I**T seems to me that the electric wind has not been sufficiently distinguished from another impression like that of a *tickling*, which the electric fire excites in us; nor do I know that any just cause of it has hitherto been assigned. I. When, in very proper electric weather, you immerse your face, or the back of your hand, into the brisk electric atmosphere of a sufficiently ample body, whether insulating or deferent, you feel a kind of tickling on your skin, the same as if you moved through cobwebs; that is to say, the excessive fire which actuates the above atmosphere, drives the natural fire from the contiguous air to your face, and this fire, in getting into your skin, produces the above sensation of tickling: if the body near which you stand, be electrified by deficiency, then the fire within your skin flows to the external surface of it, in order to accumulate itself on it, and form an excess; now, this fire produces the same kind of feeling, whether it issues out of your body, or gets into it. II. Hence it comes, that when you are immersed in a given part of a quiet electric atmosphere, you feel nothing; but in the very instant that, by extracting a spark, this atmosphere becomes annihilated, you feel the above sensation of tickling renewed, in consequence of the fire which now issues out of you, or gets in. III. If you bring your face so near the electrified body, that the fire may pass divided from the one to the other, the tickling will grow stronger,

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and more pricking, whenever the fire will begin to be somewhat more united, and will want only one degree more of union to form real sparks. Therefore, we may distinguish two kinds of tickling; the one arising from a simple atmosphere, the other from an actual electricity, which diffuses itself in a divided state. Upon the whole, we may lay this principle down, that *sensations of an electrical tickling will arise from the passage of a certain quantity of electric fire into your skin, or out of it; and that this sensation will be weaker, in proportion as the division of such fire will be greater.*

781. From the above tickling, the electric wind must absolutely be distinguished, *which is a real current of air, driven from a point, either annexed, or presented, to a body strongly electrified.* I. The coolness we feel on our hand, supplies us with one proof, that this wind really is a current of air, since it is the property of air to cool when it is renewed; both, because to an heated air, succeeds another which is not so, and because to an air rendered damp by the contact of our body, succeeds another which, being more dry, is more apt to promote our perspiration, consequently to cool us. II. The different kinds of motion which this wind may create, afford another proof of its being a mere current of air; it, for instance, drives away the smoke of a candle lately extinguished. If, while you are insulated, and communicate with a body strongly electrified, you rapidly present a bodkin to the flame of a candle, the flame will, in the first instant, be driven by the wind which the bodkin produces; though this will take place, as I say, only in the first instant, because the electricity of the bodkin will soon dissipate through the deferent flame, and the wind will of course immediately cease; there will only remain on the bodkin a small electricity, which, instead of driving the flame away, will attract it, and through it intirely escape. III. The motion of small wheels driven by an electric wind, is still more permanent and significative, because the electrical fire escapes in a much less quantity through them, than it did through the flame. I make such wheels in a very expeditious manner: I cut a cube of cork two lines broad, and to each side of it I adjust, with some spittle, a rectangular piece of wafer, an inch long; I then insert a needle through

through the cube, and suspend it by this needle, either with the armature of a load-stone, or with a long thin thread; then presenting the electrified bodkin to the sides of the wings of the wheel, the latter continues to turn round, and it changes its direction whenever I change the direction of the bodkin.

782. *To the air driven directly, and in the direction of their own axis, by the above bodkin, or by a long needle, another succeeds along their sides, so that, besides the direct wind from the needle, another is to be distinguished, which may be called a secondary lateral wind.* I. I present the wheel to the sides of the needle, at the distance of one, four, or even five inches from its point, and it then turns pretty rapidly; I afterwards present it to the other side, and the wheel changes its direction.

783. I usually employ the following experiment, both in order to make the direct wind visible, and demonstrate the necessity of the lateral wind. I pour oil into the bottom of a china dish EF (Pl. X. fig. 7.) on the edge of it, I place a lump of soft wax, on which lies a rod inclined towards the surface of the oil, but which neither touches, nor can attract it, lest it should thereby become incapable of producing the wind; I then electrify the rod, and presenting my finger to the surface of the oil, at the distance of two inches from the point, I see the surface of the oil depressed, and ruffled by the impulsion of the wind that arises from the rod. Now, who does not perceive, that as the motion in the insulating oil is no more than a progressive motion of it from the point, so the motion of the insulating air is no more than a like motion, or pulsion of the same; it is not true, therefore, that an *effluent* electric matter be the immediate agent that drives away the smoke, or puts out a small candle, and that it forms by itself the electric wind; an opinion this, however, in which I see many a philosopher persist.

784. I spread some shavings or scrapings of a beaver hat, on the surface of the oil, and presenting my finger, as usual (the dish is of itself insulating, and, besides, lies on an insulated stool) and attentively watching for the first motion produced by the electricity, I see that the rod drives the dusty pellicle of the oil, directly to the distance of about two inches, and the effect of it soon extends

tends all round. This motion of the surface of a tenacious heavy substance like oil, which extends to the distance of two inches, makes me understand easily how light moveable air may be driven so far as to the distance of a whole foot.

785. The beaver dust which is spread on the oil, forms upon it small clots, the different complicated motions of which are pretty agreeable to observe; they are first driven from the point of the bodkin, to the distance of several lines from it; then they are buried under the surface of the oil; when at a certain depth, they bend their course back, and, in short, by all their different motions, evidently shew the cause that produces them.

786. Points, at the same time that they drive the air forwards, are themselves driven back. Mons. Jallabert was the first who observed this repercussion of points, which the Abbé Nollet afterwards found to be inconstant. But this very inconstancy is subjected to some laws: *in order that a point sufficiently moveable may retrocede, it must be animated by a strong electricity, and a strong electric current must actually issue out of it, or get into it. But if the electricity of the point be only what we called in former chapters, a pressing electricity, then the point will be attracted by the body unequally electric that stands near it; and in this case, the general law of the attraction of bodies, will continue to obtain, for the suspension of it in the above experiment, arose from the current of the electric wind.* Therefore, if to an animated system, you fix the sharp support B (Pl. IX. fig. 3.) on which the incurvated needle EFCD stands equilibrated, you will see the latter turn in a direction contrary to that of the electric wind that rises from it, and exhibit in the dark a circle of light, the rays of which will be the more or less extended, according as the needle, or its support, shall communicate either with the Chain, or with the Machine, and, consequently, produce either a brush, or a star. If you hold the support in your hand, and present one of the points of the incurvated needle either to the Chain or to the Machine, the needle will still continue to turn; but then it will only exhibit an arch of light in that part of the circle it describes in which its points alternately give fire to the Machine, or receive it from the Chain.

C H A P. II.

Of the reason of the electric wind, and of the retrocession of points.

787. **T**HE electric fire runs out of points, or gets into the same, with such velocity, that the direction of it cannot be discerned either by the naked eye, or with the assistance of any glass. When the incurvated needle (Pl. IX. fig. 3.) is annexed, or presented, to a system animated by a pretty strong electricity, it soon assumes a motion so rapid as to make it exhibit in the dark, a continued circle of light, so that it is not possible to discern at what part of any given turn the point really begins to lighten, and in what direction it proceeds while it continues so to do. If we experiment by day-light, and consequently can see the needle, the impression of its electric light on our eyes, is less vivid, and less permanent; yet the rapidity of the needle is such, that instead of perceiving the direction according to which it turns, we only perceive a continued circle of shade; this sensation of shade, I impute to the weakness of the transitory impression, though, at the same time, the rapidity of the wheel makes this shade appear as if continued through the whole circle; it is, therefore, impossible, under such circumstances, both to discern the direction of the needle, and to receive impressions exact enough of any particular portion of its course, to be thence enabled to perceive whether the perceived light gets into it, or issues from it; nor could a microscope be of any service to us, the space which will be seen through it, will indeed be increased, but the time in which this space will be ran through, will still remain the same.

788. Since I am on this subject, I ask leave to dwell on it a little longer. In the first place, we are accustomed to see that liquids springing from the hole of a vessel, divide and spread themselves; it is no wonder, therefore, that the Parisian academicians, persons so learned in any other branch of human knowledge, though not so much conversant with electrical matters, when they saw the effluvia of the electric fire more dilated near
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the rubbed globe of glass, and more united near a key that was presented to it, thought that the electric fire proceeded from the key to the globe. Introduce persons not conversant with experiments on light, into a darkened room, in which a ray of light is admitted, howsoever refracted, provided it exhibits the appearance of a cone, and they all will agree in answering, that the ray proceeds from the apex of the cone, while it may very well happen that it proceeds from its base; now, the same is likely to have happened with the above Academicians, with respect to the direction of the fire, in their different experiments. The electric fire deposited by the hand on the glass globe, cannot unite again on the surface of it, which is insulating, but must, from the distant points of this same surface, run to the key through distinct convergent lines; and the observer, if unacquainted with the whole complement of the facts, will be led to judge, not according to truth, but to his own experience, that the fire springs from that part to which it yet really runs, merely because he sees it there to be more dense and sparkling.

789. The same is to be said of the spurious brush, into which the star will be transformed, in circumstances like those related in num. 119. Whoever observes this brush for the first time, is induced by habit to judge it springs from the point; but whoever afterwards makes the considerations expressed in num. 122. soon sees himself necessitated to alter the rules he deduced from custom, and to substitute others more conformable to the complement of the facts, with which he has now become acquainted.

790. In order to make the subject more thoroughly understood by the reader, I shall now proceed more minutely to examine the different accidents of the motions of the electric fire, when it forms either the brush, or the star. *The rapidity with which every particle of electric fire issues out of a point, or enters into it, is not only considerable, but the rapidity with which these particles succeed each other, is also very great.* Whenever I annex a point to an electrified system, the divergence of the electroscope lessens proportionally to the greater sharpness of such point, proportionally to the distance to which this point reaches beyond the limits of the atmosphere of the system, and proportionally to the vicinity of that same point to
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strange bodies. I sometimes annex a wooden frame to the hook Z of the conductor Y, on which I have stretched some gilt paper, and from the latter I draw sparks with the knuckles of my fingers. If instead of my knuckles, I use a sharp point, which I move with sufficient velocity towards the gilt paper, to bore a hole through it; then this hole is effected without drawing any spark; the reason is, that the velocity with which the fire in the paper (which arises from the conductor) throws itself into the point, is such, that when the point reaches the paper, the fire has already passed intirely into it, and there remains none in the paper, or in the conductor, to produce a spark. I observed, in a former place, that when I discharged a coated sheet of glass with a point, though very sharp, which I rapidly moved towards it, there always remained in the sheet a pretty strong spark; but this only shews, that the velocity of the fire is limited, and the above experiment of the gilt paper shews such velocity to be extremely great.

791. *The air driven from an electrified point, moves forward to a pretty great distance from this point, it being pushed forwards by other air, which continues to be driven from it; and it is impregnated with the excessive fire that flows from that same point, if it be electrified by excess, or deprived of its own fire, which it gives to the point, if the latter be electrified by deficiency.* If you insulate yourself at the distance of about a foot from a flat, or but little convex, part of a system animated with electricity, the atmosphere actuated around it will, as we have formerly seen, introduce a contrary electricity into that part of your body which is immersed in it. But if you insulate yourself, facing a point that springs from such system, the wind that blows from that point will introduce into your body the same kind of electricity as that in the system, and introduce it at the distance of five, or even six feet; which introduction will be the quicker, as the distance will be less, and the point sharper, and extended farther beyond the limits of the atmosphere of the system.

792. If you take a rolled piece of metallic foil, and keep it insulated by a long, very thin, and dry silk thread, and present it to an electrified flat body, it will hang according to its natural direction,

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tion, because bodies attract each other only when contrarily electrified, and repel each other only when similarly electrified; if you touch the rolled leaf, it will fly to the system, as it is now become contrarily electric. But if you now present the same to a point, even three feet distant, you will see it repelled by it; touch it, and it will hardly have begun to move towards the point, when it will fly from it.

793. Hence we see, that one of the greatest difficulties that remained to be explained concerning electricity is removed. Indeed it was a thing very difficult to conceive how it came to pass, that while a spark, which appears so active, can only pass through air, to the distance of an inch, the thin effluvia from an electric brush (which may be brought to such tenuity as, at the distance of an inch, to become wholly invisible) fly by this means to the distance of several feet. The fact is, that they do not run to such a distance, driving the air all along, by dint of their own continued motion, but are transported so far, by the general motion which the electricity of the point communicates to the ambient air, and continues to maintain in it.

794. In the same manner, if the point to which I present my hand, at the distance of three feet, be electrified by deficiency, the fire does not run directly from my hand to supply this deficiency, through this interval of three feet; but it is the natural fire of the air, which, from the sides of the point, continually flows to the apex of it, that diffuses itself into it; and it is this air, thus deprived of its natural fire, which, being successively driven towards my hand, as it were carries the deficiency of the point to it, so that my body, if I stand insulated, becomes electric by deficiency, because continually new particles of fire diffuse themselves from it into the air which is brought by the above wind, deprived of its own natural fire. Hence we may also deduce a solution of another difficulty still more weighty, concerning the electric wind itself, which, as we see, equally blows *from* points electrified by excess, and by deficiency.

795. *The natural fire which, from the air contiguous to a point diffuses itself into it, must needs drive forwards the air which lies around*

around the point, and repercuss it in the same manner as it happened when the point was electrified by excess, and the electric fire rushed from it into the contiguous air. Universally, whenever the electric fire moves through substances in any degree resisting, it drives their particles on all sides, but especially in that direction in which it meets with the least resistance. This truth necessarily follows from the natural expansive force of the electric fire, and is in all cases warranted by experiment.

796. Now, if the electric fire, when passing condensed to a great degree, through bodies or mediums, has the propriety of driving their particles into contrary directions, it certainly must equally exert this propriety, both when it passes from the air into the point, or from the point into the air, and drive them both into opposite directions, after the same manner as in a former experiment, the pieces of a little glass tube which contained a certain quantity of water, were, as well as the water, driven into opposite directions.

797. The reader may also here recollect the experiment before related, of the quire of paper through which a discharge was sent; whatever the direction of the spark was, either upwards or downwards; the brim of the hole was turned upwards in all the leaves except the last, in which the bur was turned downwards. This experiment shews that the center of the action of the fire, whether it issues from the point, or gets into it, lies at a certain short distance from the top of this point; and the fire, from such center, drives the particles of the resisting medium on all sides.

It, therefore, appears, that the celebrated question, why points drive the air forward, whatever the direction of the fire may be, is reduced to the universal fact, viz. that the electric fire drives the resisting particles of the medium through which it moves into contrary sides; we, in fact, see, that the electric fire, in all cases, drives both the air and the point; but for the more complete explanation of this subject, there remain a few experiments more to be related; which will be done in the following chapter.

C H A P. III.

The reason of the brush, and of the star.

799. *AN* homologous atmosphere, if it sufficiently surrounds the point, suppresses the wind, the star, or the brush, and hinders the electricity, whether by excess, or by deficiency, from dissipating itself through this point. Present a glass tube strongly electrified to a point annexed to the Chain, so that the atmosphere of it amply involves the point; or to a point adapted to the Machine, present a stick of sealing-wax strongly electrified; and the wind will immediately cease, the star or brush disappear, and the electrosopes soon acquire their greatest divergence.

800. Nor is it necessary that such bodies as are presented to points, should be actually electrified, it is enough that they be insulating, and sufficiently extensive. I bring a glass spoon very hollow, near to a point, so that the apex of the point be inclosed within the brim of the spoon; at the same instant the light disappears, and the electricity in the system to which the point is annexed, increases; the reason of this is, that the spoon stops the air either loaded with the fire of the point, or deprived of its own; which is sufficient to suppress any vivid electricity from the point.

801. Hence it also happens, that a point turned backwards, and directed towards the system itself to which it is annexed, ceases to give, or to draw, any fire.

802. *An homologous electricity obliquely directed to a point turns the wind, the brush, or even the star, to a contrary side.* I adapt to the conductor Y (Pl. X. fig. 8.) a rod which I fasten to it in an oblique situation; and I observe, that in proportion as I make the angle comprised between it and the conductor more acute, the brush and the wind deviate more from the axis of the point towards the opposite side.

803. Thence I have understood an irregularity, which I observed with regard to the incurvated needle EFCD (Pl. IX. fig.

fig. 3.) I placed it on an electrified system, and with some sealing-wax I fixed its extremity D; then presenting a little wheel laterally to the prolonged axis of the point E, I was amazed to see that the wheel either did not move, or moved in a direction quite contrary to what I expected. The fact is, that the atmosphere of the body FC of the needle, made either the wind, the brush, or the star, deviate considerably; so that in order to make the little wheel turn, it ought to be presented, not to the prolonged axis of the point of the needle, but to the wind itself, which, owing to the particular shape of the whole needle, was somewhat obliquely directed.

804. Thence we may also understand why, when we insert a point through a small tube of glass, the orifice of which extends somewhat beyond the apex of the point, and present our finger to it, the brush, or the star, degenerate to a number of brisk small sparks. The electricity from the point soon fills the tube, and there arises all round the point, and close to it, an homologous electricity which soon suppresses the electricity of it.

805. An homologous atmosphere which only extends towards the system to which a point is annexed, increases the wind, the brush, or the star. Adapt a point to the conductor A₃B (Pl. VIII. fig. 8.) let three men rub a glass-tube each, and jointly present it, the one to the back part of the conductor, and the two others, along each side of it; if the operation be made in the dark, you shall see a brush spring from the point, though there might be before no electricity in the conductor; if the latter be electrified in any degree, you shall see the brush sparkle with the more briskness.

806. *Contrary* atmospheres *similarly* applied, produce contrary effects; applied after a contrary manner, they produce similar effects. I. A contrary atmosphere directly applied to a point, quickens the wind from it, as well as its brush or star, and increases the dissipation of its electricity. II. Being obliquely presented to the point, it draws to itself the wind from it, as well as the star, or brush; and in proportion to the intensity of its oblique action, it quickens the dissipation of the electricity. III. Being presented to the surface itself of the system to which the point

point is annexed, it retards and lessens the dissipation of its electricity.

807. These effects of contrary atmospheres I content myself with mentioning, because they are manifest; besides, the experiment formerly related concerning homologous atmospheres, would alone suffice to lead us to the discovery of the following important truth, viz. *that the dissipation of the electricity of bodies through points, is owing to the electric atmospheres of those bodies, in consequence of the reaction which they exert against them; which reaction is much greater along the flat parts of such bodies, than around their sharp parts, or the points annexed to them.*

808. I shall examine, in the first place, the case of a point annexed to a system electrified by excess, which throws a brush; though I have in a former place laid down the principles on which the above explanation is grounded, yet, in order to procure the reader a greater evidence on the subject, I shall here lay them down again. First, I observe, that the excessive fire which flows out of a point A (Pl. IX. fig. 1.) of a right line BC, or of a plain surface, does not actuate an electricity by excess in the ambient air, by diffusing itself into it, through a right line AD, or by spreading itself through it to any distance, along any lines AE, AD, AF, but that it stops in A on the surface of the body, and that from thence, without leaving the place A, it introduces a particular kind of tension into the fire proper to the air around this body; these facts, I think, I have sufficiently demonstrated in the third section of this book.

809. I observe, in the second place, that this tension I mention gradually lessens in a certain proportion to the different distances of the body, so that it ceases somewhere, for instance, in EF, in consequence of the *vis inertiae* which is inherent in the electric fire, as well as in other substances.

810. In the third place, I observe, that such tension will take place not only in the fire proper to such portion of air AD as is directly opposed to the point A, and in the lateral strata of it AE, AF, but will communicate itself backwards through this air, after the same manner as any kind of motion is propagated through fluids; on which the reader is desired again to peruse the paragraphs 452—455 of this work.

811. This

811. This important observation supplies the solution of the intricate knot or difficulty we are now handling. I. The fire proper to the ambient air, being brought to a particular degree of tension by the electricity which is actuated in A, exerts a reaction against that same electricity. II. And that electricity which is actuated in A, must needs, by the means of the tension which it introduces into the fire proper to the ambient air, and which it propagates to all sides, also exert a reaction against the fire which seeks to issue from the adjoining points G, I, K, H, B, C, &c.

812. Now, the conclusion I draw from the above principle, is the following. If the surface, however varied, of a system be conceived to be divided into a number of equal exceeding small particles, and the adjacent atmospheres be conceived to be divided in the same manner, the excess which is exerted from each of these particles, experiences a reaction proportioned to the number of these different atmospheres which co-operate in repercussing it, to the less obliquity with which they act, and to the less distance from which they do so. Therefore, the whole reaction experienced by the excess that seeks to rush out of narrow hollow places, will be relatively infinite (the reader is desired here to recollect the former experiments on the electric *well*) because, though the number of the atmospheres may grow less as the cavity grows narrower, yet they then act more directly against each other, and from less distances. III. This reaction will, therefore, lessen as the cavity will grow wider. IV. It will lessen still more when the hollow place will become convex. V. And its value will be infinitely small when the convexity will become infinite, and, besides, reach beyond the limits of any atmospheres belonging to other parts of the system; that is to say, when the above convex part will be turned into a very long sharp point.

813. In the fig. 1. pl. IX. let ABC be the line that would be represented on the surface of a long sharp rod by a section made parallel to its axis. I. Let such line be conceived as divided into as many particles Bb, equal to the tip C of the point of the rod, as it may contain. II. From all these points let the perpendicular equal *electric lines* be excited, BF, bf, &c. CG, DA, EC, and then

then the half part of the angle ECQ will express the superior obliquity (relatively to the obliquity with which the lateral atmospheres act against each other) with which the atmospheres around the point act against the excess that seeks to rush from it; which, as the reader may easily perceive, is exceedingly small.

814. If we pass to examine the number of all those atmospheres, actuated around the superficies of a point, and which obliquely direct their reaction towards the excess which endeavours to issue from the top C of it, we shall find, first, that the number of these superficial atmospheres will gradually lessen near this top, in proportion as the latter will taper to a sharper point, and the geometrical proportion followed by such decrease might, if proper *data* were assigned, be easily investigated and discovered.

815. Whence, if we finally join the consideration of the small opposition experienced by the excess that endeavours to issue out of the top C , in consequence of the great obliquity of the lateral atmospheres, with the consideration of the small number of the same, we shall easily see how it comes to pass, that the great reaction exercised by electric atmospheres against the excess which endeavours to issue out of the plain surfaces of a body, must make a great part of the redundant electricity in that body rush from a point annexed to it, where this reaction or opposition from atmospheres is much less, so that a brush takes place on such point, thence also arises an electric wind, and a repulsion of the point, which always is a consequence of this wind.

816. This efficient cause of a brush is the same as that which produces sparks (and as we shall see hereafter, the same with that of the other electric signs) inasmuch as both are caused by the reaction of the electric atmospheres of the body on which they take place, against the fire in that body, which makes sparks leap from plain, or but little convex, parts of the same, or a brush spring from a point annexed to it; however these sparks and brush differ from each other in this important respect; the former only rushes out, when a strange body, for instance a finger, is approached, and thus lessens the reaction of the atmosphere against the place whence the spark we suppose is thrown (which it effects by shortening the column of air contained between it and the body) whereas the
shape

shape alone of the point from which the brushes rushes out, is able to lessen the re-action around it, so that the fire continually sent to it from the electrified body, may successively run against the column of ambient air, however extended, and blow it away while mixing with it.

817. The same cause may also be assigned to all the accidents of the brush, and first to the direction and shape of the stream of fire that forms it; the lateral atmospheres of the point A C B (Pl. IX. fig. 1.) exercise an oblique *re-action* along the spaces C E H, C G I, by virtue of which, I. The brush is confined to the space H C I. II. The electric wind blows chiefly in that space H C I. III. and the point is also repercussed in that same direction, which is owing to the air being driven towards opposite parts, in the direction of that line, along which the stream of fire is determined to flow, by the re-action of the lateral atmospheres. In fact, as a strange homologous atmosphere presented sidewise to a brush, suffices to make it deviate towards the opposite side, so the oblique action of lateral equal opposite atmospheres will have the effect of retaining the brush on the prolonged axis of the point, and settling it there.

818. The truth of the cause to which we here impute the narrowing of a brush which springs from a sharp point, is confirmed by the divergence a brush assumes, when springing from a blunted point, because on such points several distinct atmospheres may take place, which thus reciprocally confine the brushes or fire which they endeavour to raise within the body of the point. Instead of a point, make use of a brass rod, a line and an half thick, the top of which is quite flat, and you shall see the brushes spring only from the circumference of this top, and spring very divergent and widely spread, being continually repelled by the atmospheres that take place on the flat surface of the same.

819. Now, in comparing the narrow brush which springs from a very sharp point, with the several small ones that spring from a blunted point, we might, if we were to trust our senses, be led into an error concerning the real quantities of fire that are thrown out; the numerous brushes that spring from the circumference of a blunted point, seem to indicate a dissipation of fire much more

copious than can well be made through the single narrow brush that springs from a sharp point; but experience teaches us that the case is quite otherwise. I insulate myself, and communicating with the Chain, I strike a sheet of gilt paper, stretched on a wooden frame, first with a needle only, then with two, with three, with four, and with six, taking care that the points of all these needles be in the same plan; and in proportion as the needles I use are more numerous, and form a thicker bunch together, I give the gilt paper a larger spark. This aggregation, or bunch, of points, may very well be considered as one blunted point; and in proportion as the width of its top increases, the spark also increases which I give through it to the gilt paper; now, with a sharp point, I never can give any; a certain indication this, that through this single point all the fire in me escapes in an instant, whereas it does only dissipate somewhat slowly through the aggregation of points.

820. In attentively comparing the different circumstances which accompany the narrow brush that springs from a sharp point, with those of the ampler or compounded brush that springs from a blunted point, it will not be difficult to remove the difficulties that may arise from their different appearances. In the former brush you see a continued united stream of fire; in the latter, you only perceive a kind of tremor, or quivering motion, and the flashes of the fire that form it are alternately shortened and lengthened. If you consult your hearing, you will hear the sharp point emitting a continued hissing, the blunted one only a rattling, formed (as we observed in the first section) of successive distinct small cracks. If you insulate yourself, and then attempt to draw fire from a blunt point, you shall find that it is not able to send fire to you, but from a less distance than a sharp point.

821. All this is intirely conformable to theory, and to what has been above said with regard to the different degrees of reaction exercised by atmospheres; which reaction depends on the shape of the bodies around which these atmospheres are actuated.

822. But

822. But when the electricity is very intense, another seeming contradiction takes place; in such case, brushes arising from a blunted point spread very wide, whereas those from sharp points are confined to a still narrower space, they even lose their length, and are at last changed into imperfect short brushes, or stars. But it lies in my power to restore to such brushes their natural form; I only need put a piece of paper, or a bit of soft wax between the ring Z of the conductor Y (Pl. I. fig. 1.) and the hook by which the point hangs to it; the deceiving star then immediately resumes the appearance of a real brush; or I only need leave rubbing the glass, and, at the same time, that the electricity grows languid, the brush lengthens itself.

823. This transformation of a brush into a star, often happens even in the blunt points annexed to the wires with which I explore the electrical state of the atmosphere, when flashes of lightning take place; this proceeds from the vehement electricity which then arises in each of the many points that may be supposed to form the blunt point, and is an effect which the milder electricity of a conductor cannot produce upon more than one.

824. As the shortened brush hisses more strongly than a long one, so it exhibits in its short tract a more intense light; it drives the air to a greater distance, after it has impregnated it with its own fire. It is evident, that the shortening of the rays arises from both the greater density and greater velocity with which the fire rushes from the point; that is to say, as an aggregation of any corpuscles whatever, penetrates the less deeply into water in proportion as this aggregation is more compact, and strikes the water with more force, so, the more the substance of the electric fire is united together and the more rapidly it moves against the ambient air, the less deeply it divides it, and penetrates into it; though it drives it forwards to a greater distance, and more quickly escapes away in company with it, by virtue of the motion it has communicated to it: now, when the electric fire moves by virtue of this latter kind of motion alone, it even makes no impression on our eyes, and escapes in an invisible manner.

825. The *unity* to which these explanations which rise from the fact itself, bring the various accidents of the brush, I consider

as a new proof of the truth of the theory above laid down with regard to that electric sign. I shall now conclude on this subject, with relating an observation of Mr. Kinnerley (Franklin, p. 337.) which appeared at first a bare unconnected fact, and which, the theory above being understood and admitted, becomes an observation pregnant with luminous consequences. Let an insulated person hold a point, the apex of which is turned outwards, and remains near his body, and it will only exhibit a very scanty languid degree of light; but as soon as the person stretches his arm, the light greatly increases. Mr. Kinnerley imputed this to the more copious electricity which was diffused in the air near the body of the person; but now, when it is demonstrated, that the *actuating* electricity remains confined to the surface of bodies, this explanation cannot hold; it is, therefore, clear, that the fact must be attributed to the *re-action* of the atmospheres, as explained above.

826. The same observation, which is common both to points electrified by excess, and to those electrified by deficiency, leads us to apply the same theory to the latter that has been hitherto advanced with regard to the former. In points annexed to a system whence the electric fire is drawn, the direction of the actuating force is inverted, and that of the actuated fire must of course be also inverted. I. Correspondently to that portion of electric fire which is drawn from a part of a body, either plain, or but little convex, the natural tension in the air contiguous to this part must be relaxed. II. This relaxation is propagated to a certain distance, with a suitable diminution, and is also turned laterally, and to all sides obliquely, against the ambient air, contiguous to the same body; it, therefore, opposes a resistance to the force that endeavours to draw the fire from that body. III. Now, if we apply to this case what has been above said with regard to the brush, it is plain, that the reciprocal opposition we mention, will be greatest and relatively infinite in narrow cavities; that according as those cavities shall increase, the opposition will lessen; when the cavities become plain surfaces, it will lessen still more; when they become convex, it will be still less; and when infinitely convex, that is, when they become changed into very sharp points that reach

reach quite out of the atmospheres of the body to which they are annexed such opposition will become infinitely small; therefore the force that will draw the natural fire at the apex, will become relatively infinite; that is to say, this force will be able to draw the natural fire from the contiguous air, and thus a star will take place.

827. The characteristic mark of this electric sign will, in proper circumstances, be the shortness of its light; but before I treat this subject, I must anticipate a difficulty which will naturally be proposed against the above explanation of the brush and the star. Hitherto I have only spoken of points annexed to electrified systems; but the fact is, that a point presented from the ground to the Chain, will exhibit a star just the same as if it were annexed to the Machine; and a point presented to the Machine will throw a brush in the same manner as one annexed to the Chain; now neither the fire in the air that surrounds the man who presents a point to the Machine, is brought to any particular tension, nor is that of the man who presents a point to the Chain brought to any degree of relaxation; it, therefore, seems, that in both these cases the efficient cause fails, which we have above assigned to these two electric signs.

828. But this difficulty cannot be proposed by persons who have attentively considered the theory of electricity. The natural tension of the fire in the air that surrounds my body, may very well be either excessive, or deficient, *relatively* to the tension of the fire around the apex of the point, though there is in myself no absolute electricity. When I present a point to the Chain, this point becomes immersed in an air, the fire of which possesses a vehement tension, and is strongly vibrated towards the point; therefore with regard to such tension, the natural tension of the fire around my body, is a relative relaxation. In the same manner, when I present the point to the Machine, it finds itself involved in an air the fire of which is much relaxed, especially near the Machine, and is strongly solicited towards it, as the latter is in a wanting state, &c.

829. These principles being laid down, it becomes pretty easy to determine the circumstances in which the star keeps its characteristic appearance, viz. that of exhibiting short and scarcely discernible rays.

rays. I. On a very sharp point, whether it be annexed to a system electrified by deficiency, or presented to one electrified by excess, there always appears a real star, whatever may be the distance (provided it be not too great), and the shape of the particular part of the system towards which it is directed. II. The same is to be observed with regard to blunt points. III. Universally, whenever a point draws only such fire as lies in the contiguous air, this fire exhibits the appearance of a star; it can present the appearance of a brush, only when it leaps to the point from the electrified body itself, and unites again near it, dividing the air by dint of its own motion; now, this can never be the case with a sharp point, because, while it is bringing to the electrified body, it so readily draws the fire in it, that with whatever rapidity it may be presented, the whole excess in the body is absorbed by it, before it comes to a distance sufficiently short to produce a brush: it is only a blunt point, or one which is imperfectly deferent, that can come near the system, and yet allow a pretty considerable portion of its excess to continue; which excess leaps to it, by virtue of its own motion, as soon as the distance is become short enough, and then exhibits the appearance of a spurious brush.

830. The above explanations concerning blunt, or imperfectly deferent points, may be applied to the different experiments in which the Parisian Academicians thought they saw the electric fire run from points to rubbed glass globes, in the same manner as it would to a conductor negatively electrified (Nollet, *Lettres sur l'Électricité, seconde partie*, pag. 253.) The top of a finger, the ring of a key, the edge of a silver crown, which they presented to a globe, where more or less blunt bodies; a piece of green wood, the extremity of a wetted rope, a piece of pasteboard, &c. were bodies imperfectly deferent; and the head of the metallic square rod, eight lines thick, which they used, had in its angles and sides a kind of aggregation of points more or less blunt. All these bodies, besides, were presented to the globe at a distance probably very small, since it is said of the square rod, that it was not more than half an inch distant from it (pag. 254.) it is not, therefore, to be wondered, that if the rays of the electric fire, which the
hand

hand left on the globe, leaping through the inconsiderable stratum of air before them, and uniting together on the above mentioned bodies, exhibited spurious appearances of brushes; which induced the Academicians to judge that the fire, instead of proceeding from the glass globe to the bodies, diffused itself from the bodies into the globe.

831. But amongst the experiments which the Abbé Nollet made on this subject, in the presence of the Academicians, there is one which alone suffices to resolve the difficulty, and show the real direction of the fire when it forms these spurious brushes. We took, says the Abbé, pag. 254, num. 16, a square iron rod, six feet long, and eight lines thick, and employed it as a conductor. The brush having spontaneously spread itself at the remotest extremity of the rod, always grew larger and more luminous when somebody presented his hand, his face, or any other broad part of his body to it (num. 18.) But when (num. 19.) the same person presented the top of his finger, or a piece of metal of a like shape, at two or three inches distance from the conductor, then the brush which issued from one of the angles, directed itself towards the body that was thus presented, and its rays, naturally divergent, bent themselves towards it, as if seeking to embrace it. If the finger, or piece of metal, was presented still nearer, the rays at last contracted themselves so, as to form a single stream of a denser and more inflamed matter.

832. In this experiment, the Academicians saw with their own eyes, and confessed the unity of the direction of the fire when it gathered on the finger, and there exhibited the appearance of a spurious brush; now the same appearance must also have taken place when the finger was presented from the ground, either to a rubbed glass globe, or to a broad flat part of an electrified body, or conductor. The only difference was, that in the last cases, the rays of electric fire sprung divided from the larger surfaces of the bodies we mention; whereas, when the finger was presented to the angle of the conductor, the rays only became again united together, and then the spurious brush took place, into which the true one became transformed, by its divergent rays being thus made again to gather and unite together.

833. The

833. The observation added by the Abbé, in the following paragraph, that the finger, or piece of metal, when thus held opposite to a brush, appeared covered with luminous points, whence rays proceeded almost similar to those of the brush, is of no moment; those rays were nothing more than the rays themselves of the brush, which, by bending themselves towards the finger, and converging to it, may, nay must, have covered it with luminous points.

834. I shall only add, as a conclusion of this subject, that, of the experiments made by the Academicians, which is contained in numb. 31. Whenever sparks were excited between a conductor and a body not electrified (these are the words of the Abbé), if both were of a considerable bulk, and every circumstance favourable to electricity, we observed that the stream of fire came as well from the one as from the other. With regard to this assertion, I should think it an affront to the visual faculty of the reader, were I to offer a single word.

835. I have rather to conclude this chapter with assigning the cause of the short rays of a real star, and the long rays of a real brush; this cause manifestly appears from the various circumstances in which, as I have before related, these electric signs retain their real appearance, or assume fallacious ones. The star appears, whenever a point draws the electric fire from the neighbouring air; now, *the force with which the insulating particles of air retain their own fire (which never quit hold of it, but like other insulating substances, from a small depth) is the cause of the shortness of the rays of such star.* The brush appears whenever excessive fire is thrown into the neighbouring air; now, *the force with which the electric fire runs forwards through this moveable contiguous air, is not by affixing itself to it, but by dividing it; hence the extension of the rays that form the true brush.*

SECTION

S E C T I O N VI.

On electric motions.

836. **T**HE electric motions by which bodies approach each other, or recede from each other, in air which is free from electrified vapours, may be reduced to the two following laws. I. Two bodies similarly electrified, recede from each other in proportion to the sums of their electricities, directly, and of their masses, inversely. II. Two bodies contrarily electrified, move towards each other, in proportion to the sums of their contrary electricities, directly, and of their masses, inversely. In air impregnated with electrified vapours, the two aforesaid laws are liable to an exception, but which yet is of a pretty simple nature. The laws concerning more intricate motions, are only compounded of the laws above. However, as this subject is very extensive, and since with the explanation of the above laws, we are to combine the extensive law of the *vindicating* electricity, which will be laid down hereafter, I propose to divide the present section into two parts.

First part of the Sixth Section.—On the electric motions of deferent bodies.

C H A P. I.

On the law, and measure of the retrocession of deferent bodies, in open air.

837. I. **I** Unite together the angles of a silver leaf A, and keep them thus joined with a long thin silk-thread; the other extremity of which I fasten to a hook Z (Pl. I fig. 1.) of the conductor Y, and I take care that the leaf be placed at a great distance from any strange body. I then grasp the belly of a
Z z
bottle,

bottle, the hook of which is terminated into a ball ; and with the other hand I hold, by a similar silk thread, another leaf B, exactly like the former A.

838. With the above ball, I touch the leaf A, and thus electrify it ; then, stretching my hand, I gradually carry this leaf A near the leaf B. So long as the distance between both remains greater than an inch and an half, the two leaves hang parallel to each other ; whence I perceive, that, *between two bodies, the one of which is electrified, the other not, no electric motion takes place.*

839. Another person presents his finger to the leaf B, and withdraws it as quickly as he can ; then the two leaves run and meet each other. As soon as a contact has thus taken place between them, they mutually recede from each other, and if the upper extremities of the two silk threads be joined together, the two leaves exhibit a considerable divergence ; that is to say, the leaf A with its atmosphere endeavours to drive the natural fire from the next surface of the other leaf B, into the opposite surface of the same ; whence results on this opposite surface an atmosphere by excess, which expells the natural fire from the finger that has been presented to it ; the leaf B therefore runs to this finger which now is in a wanting state, and gives some of its own fire to it ; as the two leaves are thereby brought to a different electrical state from each other, they move to each other, and by this means *becoming again similarly electrified, again diverge.*

840. IV. I remove the leaf B from the leaf A, and touch either of them, with the hook (or ball) of the charged bottle ; when both the leaves, if brought close to each other, diverge still more than formerly : the reason is, that, in the former instance, the two silver leaves diverged in consequence of the electricity of the ball, or hook, communicated to A, and thence communicated to B ; whereas they now diverge by virtue of both the remnant of the former electricity in the one, and of the whole electricity just now diffused into the other.

841. V. I now also touch the other leaf with the ball ; and when both are brought near each other, their divergence increases still more : whence I infer, that *the mutual recession of two bodies, similarly electrified, is proportioned to the sum of their respective similar electricities.*

842. These

842. These experiments must be made in very favourable weather, because otherwise the atmosphere of A may drive the natural fire of B up the silk thread by which it hangs; and then attract it as soon as it is brought near it: it must be observed farther, that in order to cause one of the leaves to run to the other, and thus draw its electricity, it must be first touched by another person, that it may through him speedily send away its own natural fire; otherwise it would not move towards the other body but with great slowness.

843. With regard to the proportion that obtains between the mutual recessions of deferent bodies, and the sums of their similar electricities, I shall, at the end of this chapter, relate a few more precise experiments. I only proposed here to consider the compounded recessions of two or more bodies similarly and equally electrified; that is to say, the mutual recession which is produced in these bodies, not only by their own atmospheres, but is in each of them modified by the action of other neighbouring homologous atmospheres, and also by the particular actions of gravity.

844. I. Even the mutual recession of two equal flaxen threads, fixed to the same hook Z of the conductor Y (Pl. I. fig. 1.) is a compound motion; because they are repelled, not only by their own reciprocal atmospheres, but also by that of the conductor: therefore, if the conductor be horizontally situated, they recede from each other in a vertical plane, perpendicular to the axis of the conductor: the reason of this is, that it is in this plane that the atmosphere of the conductor, which follows the shape of it, is least extended, and its repelling power is least: the threads are therefore carried and supported in this plane by the excess of the repulsion which arises from opposite parts of the conductor.

845. Nor do the threads diverge as much as should naturally be expected from the action of their atmospheres; the ampleness of the conductor, to which corresponds the ampleness of its atmosphere, laterally repels these threads, and thus lessens the angle of their divergence, which increases in proportion as this atmosphere is less extended.

846. From these same principles it follows, that if two threads are annexed pretty near the extremity of the conductor, they will

Z z z

deviate

deviate towards this extremity, since the action of the conductor's atmosphere will, in this case, be more extended and efficacious on the one side of the threads, than on the other.

847. Let the two threads hang from a point A (Pl. IX. fig. 12) in the middle of the lower surface of the conductor Y, which is inclined to the horizon. Let the line N A be drawn perpendicular to the lower surface of the conductor, and the line A V be drawn perpendicular to the horizon; then the two threads A B, A C, will diverge in a plane B A C, which will be contained between the two lines A N, A V: the gravity of the threads endeavours to make them move in a plane belonging to A V; the action of the atmosphere of the conductor endeavours to make them diverge in a plane belonging to the perpendicular A N; they therefore will diverge in a plane that will be in the middle between both.

848. In the figures 13, 14, 15, 16, the right line I M expresses the lower side of a cylindric conductor four or five inches wide, and horizontally situated; the point A expresses a middle point on this lower side, to which three, four, &c. threads are fastened. Let us suppose that only three such threads are suspended to A (fig. 13.) the points B, C, D, may express their lower extremities in the same manner as if they were seen projected on an horizontal plane placed under them: the two threads B D will diverge to opposite sides and deviate from the vertical line, in consequence of their being repelled by the thread C; which in its turn will be repelled by the two others; though this repulsion or deviation of the threads, will be much lessened by the action of the atmosphere of the conductor.

849. If to the point A (fig 14) four threads are annexed, the points B, C, D, E, will express the places of their divergences; two of these threads will similarly diverge on each side, and all will equally deviate from the point A; but the excess of the action that results from the length of the atmosphere of the conductor, will cause the distance of B from C, and of D from E, to be less than the distances C D, B E.

850. If to the point A five threads be adapted, (fig. 15) four will diverge laterally from A, one will diverge in a vertical plane made on I M; but two of the lateral ones C and D will diverge less

less than the two others B F, which will be supported and kept out by the atmosphere of C and F, and of D, and F.

851. To the point A (fig. 16) let six threads be suspended; they will diverge laterally, three on each side, and all six will form an exagon; only the sides B C, B D, G F, F E will be shorter than the sides D E, B G, by virtue of the increase of repulsion that will arise from the length of the conductor's atmosphere.

852. But the most striking experiment, is that represented in fig. 10 and 11 of the same Pl. IX. A, B, C, D, E, F, &c. are a series of threads, that hang parallel from the lower surface of a conductor, and are distant from each other about two inches. When the electricity is sent into the conductor, the threads move from each other, alternately parting, the one on the left, the other on the right side of the conductor, as is represented in B, D, E, H, L, N, P, R, &c. (fig. 10) and form as it were, two rows, D, H, N, R, &c. B, E, L, P, &c. Besides this distribution, and mutual recession of the threads on both sides, we may farther observe, that the threads at the extremities, B, D, &c. P, R, &c. from the middle threads deviate outwards, because there are no other adjoining threads that may repel them, in the same manner as they are repelled by these middle threads. Certainly, if we were to investigate the different degrees of action which atmospheres of different shapes exercise against each other, we would find that those compound actions accurately follow the principles laid down in the theorems of mechanicians concerning compound pressures.

853. I shall add here a very simple trial, which I have made to measure the proportion between the divergence of two threads, and the quantity of the electricity that acts upon them. I. In the middle of the hall allotted for experiments in the Royal University, I suspended with long silk strings, X Y, X Y, (Pl. V. fig. 1) a tube of tin Q q, four feet long, three inches wide, and weighing three pounds. II. To the lower extremity Q, I suspended a plummet Q R; to the point A in the middle of the length of the conductor, I suspended two very thin silver wires which I had straightened in the fire, that they might hang parallel to each other; in order to perceive their motions from a sufficient distance, I fastened two bits of paper to their extremities, and spread
a black

a black cloth on the wall, opposite to which I proposed to watch them. III. I afterwards fixed a rule OP on a steady little stool placed at a great distance, that the rule might not disturb the electricity of the apparatus; and I settled it in such a manner, that it was parallel to a plane perpendicular to the axis of the conductor, which was horizontally placed; because I knew that the wires must diverge in that plane. IV. Lastly, I placed three small cubes of lead within holes made in the rule OP, from which sprung three very sharp needles, which were to serve me as sights.

854. Things being thus ordered, I fixed a sight in L; so that my visual ray that passed through both L and the plummet QR, reached the extremity B of the wires, which hung parallel to the vertical AB: then a man insulated and electrified from the Chain (or from the Machine) touched a metal leaf placed in q with a brass rod (if he had touched the conductor itself, he might have moved it) and thus electrified the apparatus. This done, by moving the cube of lead along the rule, I placed another sight N in such a situation, that my visual ray which went through the point N and the plummet QR, hit exactly the bit of paper which then had rose to D; this done, I bade another man, who held a tube S exactly equal to the former with a stick of sealing wax, to touch the metallic sheet Q with it, and thus take away just a half part of the electricity diffused in the tube Qq: in this very instant I placed another sight M in such a manner that the visual ray that passed through the point M and the plummet, hit the point C to which the bit of paper annexed to the wire was now lowered, from D.

855. Having ascertained, by repeating several times the operation, that the sights were placed right; I suspended another plummet Al to the point from which the wires hung; I measured the horizontal distances, LR and rI; I took the distances LM, LN; and then saying, as Lr is to rI, so LN is to IG, and so LM to IH. I found IG, IH equal to the right sinuses of the femiangles of divergence DAB, CAB; whence I immediately knew the value of the chords of the said femiangles. Having repeated this experiment several times in one day, and many times more afterwards; I always found the chord of the angle DAB double

double to that of the angle CAB ; the errors were never more than a few hundredth parts of a line, and entirely vanished when I compounded together the errors that had taken place in a great number of repeated experiments.

856. Since therefore, the chord of the semiangle DAB , which proceeds from the total electricity, is double to the chord of the semiangle CAB , which proceeds from the half electricity; and since, besides the forces that suspend bodies in different arches of a circle, are to each other in the same ratio as the arches are to each other; it follows, that the force of an electricity consisting of a double excess, is double to that of an electricity consisting of a simple excess.

C H A P. II.

On the motions of deferent bodies towards each other; on the composition of such motions, with their motions from each other; and on the alterations that arise in these motions from differences in the masses of the bodies.

857. **F**ROM the very beginning of this book (num. 6) I observed that the motions by which different bodies recede from each other, were motions *of a simple pression*; by which I meant that the electricity did not diffuse itself away, either from the one or from the other system: therefore, any such motions of bodies towards each other as only take place from distances too great to allow of a real diffusion of the actuating electricity, must also be looked upon as motions *of simple pression*; these motions I propose to examine in this chapter. A metallic wire or thread, somewhat heavy, adapted to the lowest point of the conductor Y (Pl. I. fig. 1) deviates from the vertical line towards a wall that remains distant from it, two, three, or more feet, though no diffusion of the electricity of the conductor into the wall takes place. Reciprocally, a similar thread that hangs close to a wall, deviates a little from it towards the conductor, though it remains at such

such a distance, that it certainly draws no electricity from it ; this may be proved by the former experiment of the *scrutator*, or electro-scope (Pl. VII. fig. 1) which, being presented laterally and underneath to the conductor, at the distance for instance of one foot, always directed its threads in the same manner, towards the same part of the conductor.

858. Now these approaches of bodies, arising from a simple *pression*, are sometimes compounded with recessions of the same ; and they mutually modify each other. The two threads of the *scrutator* immersed in the atmosphere of the conductor, recede from each other, because both become similarly electrified, which is, by deficiency, as their natural fire is driven away by this atmosphere ; but in the mean while they also tend to move towards the conductor, which circumstance lessens the degree of their mutual recessions.

859. As the tendency of two bodies towards the same point lessens their divergence, so their tendencies towards different opposite places, increases their divergence.

860. If only one of the two bodies is attracted outwards by another strange body, they will then unequally diverge from each other ; and the angle contained between a vertical line drawn from the common point of suspension, and the thread that is attracted, will be greater than the angle contained between the same vertical line, and the thread that is not attracted.

861. With these several motions must also be compounded the accidents arising from the different gravities of the bodies with which the experiments are made ; and the divergences will be less according as the weight of the electrified suspended bodies will be greater.

862. The gravity of bodies also alters their mutual approaches or recessions. Let six threads be annexed to six equidistant points of a section X of the conductor X horizontally situated ; the electricity will tend to make these threads, or hairs, diverge at equal angles from each other ; and, in fact, if they are exceedingly fine, they will do so ; but if their weight is somewhat considerable, the vertical thread, &c. which, in order to rise, must surmount the whole force of its gravity, can no longer effect it,
and

and will bend itself downwards; the horizontal threads will also meet with a resistance that will hinder them from rising, but will be less than that opposed to the former thread; and the two oblique inferior threads will indeed meet also with a resistance from the increase of their weight, but which will be still less than that met by the two horizontal threads.

863. If two threads are of different weights, it is plain that their mutual recession will be unequally modified by their respective gravities; and it is easy to determine what will be the degree and the law of such modifications.

864. I might say something with regard to the accidents that may cause the electrical motions of bodies, to vary, and which arise from differences in the latter's surfaces, shapes, and dimensions; but I am afraid I have already too much expatiated on a subject, which, in many cases may be better understood and developed by the observer himself, than described by the pen, however exact, of a writer.

C H A P. III.

On the motions of actual diffusion in the electric fire.

865. **I** Present my hand sideways to a flaxen thread, hanging from the conductor Y (Tab. I. fig. I): this thread, when my hand is at a certain distance, runs to it, clings to it, and diffuses into it the excessive fire of the conductor. II. Also, from the ground, I present a flaxen thread to the conductor, and it runs to it, clings to it, and transmits the fire from it to my body, and thence to the ground. III. Or I present the same thread, to the thread that hangs to the conductor; when both run to each other, and the latter likewise diffuses its fire into the former. IV. If the electricity be continued, then the threads part from each other, either because they are but little deferent, or because they are too much so. If the threads are of flax, and extremely dry, they join with more difficulty; because the atmosphere of the conductor more difficultly introduces a contrary electricity into that

A a a

thread

thread which belongs to the ground : after the threads have joined together, they also preserve their electricity somewhat longer, and they part from each other only for such short times as are necessary to enable both the electricity which is retarded in the thread belonging to the ground, to dissipate, and a contrary one again to arise in it. If the threads are extremely wet, or which is better, if they are silver wires extremely thin, such as are used by Astronomers for suspending their pendulums, they join together, but soon part, and then unite again : this is because such wires, when joining and touching each other, instantly diffuse away the whole electricity of the conductor ; then they fall back, and again join together, when another excess has been accumulated.

866. Whence it appears, that the above junctions of threads, wires, or other bodies, do not continue but so long as the electric fire continues entirely to diffuse itself away through them. This same principle also serves to explain other more complicated motions, which from their successive alternations, may be called, electric oscillations ; the only difference is, that in these oscillations, the approaching and receding motions by which they are formed, are complicated together ; the following is an experiment very useful to render such oscillations conspicuous. With an whole rolled sheet of gilt paper I make a pendulum B, (Pl. V. fig. 4) which by a silk thread hangs from the ceiling of the room, and remains at a few inches distance from the conductor D. If there is no strange body near the said pendulum, and the silk thread accurately insulates it, the electricity of the conductor, however intense it may be, cannot make the pendulum move to the conductor ; but as soon as I bring a strange body near the pendulum, the latter flies to this body, and gives it a small spark ; it then flies back to the conductor and receives from it a still larger spark ; this spark the pendulum carries again to the strange body, and thus continues to oscillate ; that is to say, the pendulum B does not move towards the conductor, but when the latter is enabled to actuate in it an electricity contrary to its own ; and this it cannot do until another body is brought near the pendulum, into which the conductor may drive the natural fire of the latter.

867. Sometimes, instead of a solid body, I carry the flame of a candle near the pendulum; and this flame, even at the distance of two feet, causes the pendulum to fly to the conductor, then to part from it for a short while, and invisibly diffuse away the fire it has received from it, then to fly again to it, there to receive another spark, and thus to continue to strike against it, like a battering ram driven by an invisible force. With this latter very simple experiment, I confirm the truth of the principle that produces and governs electric oscillations, which is nothing more than a diffusion of the electric fire effected in a certain manner; and I moreover shew how the air becomes apt to favour such diffusion, first, by virtue of the rarity introduced into it by the flame, and then by that of the deferent vapours that remain spread within it, which the electric fire, while diffusing itself from the pendulum, gathers and disposes in its way.

868. The fig. 5. of the Pl. V. represents an experiment in which the diffusion of the electric fire, by means of an oscillation, is rendered still more manifest. B is a square piece of pasteboard lined with gilt paper, the lateral angles of which are somewhat blunt; A B, is a long silk thread which insulates it in the middle between the conductor D, and a strange body C, the surface of which is pretty broad: this pendulum oscillates between these two bodies, exhibiting a star on that of its sharp angles which is directed towards the strange body, and a brush on that sharp angle which stands nearest to the conductor, which in this case is supposed to be electric by deficiency.

869. Universally, the oscillations that are produced by an electric force, differ from those produced by gravity, in this; in the latter case, the motion is accelerated as the oscillating body moves towards the lowest point; but in the former, this acceleration is complicated with another acceleration towards the systems contrarily electric between which such oscillations take place, and is caused by the fire which diffuses itself from the one to the other.

870. If instead of gilt paper, a ball of metal be used, which hangs between two metal bells, the one of which is annexed to an electrified system; the ball will transmit the electricity of the one

A a a 2

system

system or bell, to the other, by striking against both alternately : balls or bells thus disposed are the means used by philosophers to be informed of the spontaneous motions of the atmospheric electricity.

871. If a series of such bells be properly framed and disposed, the first and last of which communicate with different systems ; the balls insulated between them may thus be brought to form among themselves a kind of harmony.

872. If the small bells be disposed with regard to their tones, after the same manner as the strings of a harpsichord, and communicate with each other by means of insulated small instruments like the keys of a harpsichord, such bells only will sound as shall have these keys removed from them, and thus a kind of electric music may be obtained, and an electric harpsichord effected : to this add, that care must be had to place two different keys between each pair of bells, that the oscillating ball may draw a sound from each of them separately.

873. These electric oscillations may serve for a number of entertaining experiments ; such for instance, is the *spider*, contrived by Dr. Franklin ; such is the *swing* which I mentioned in the second chapter of *Artif. Elec.* — A B (Pl. I. fig. 2) is a long thin tube of glass, to which two needles are adapted in B, which serve as an axis to it ; A and C are two gilt figures which must be without any sharp angles ; the whole is placed between four balls, two of which D, d, communicate with the Chain, and the other two E, e, with the Machine ; and between these balls, the above figures will continue to play conformably to the principles above explained.

874. These kinds of instruments or apparatus, are rather plays to amuse, than experiments to inform ; and at the same time, there are other operations which are so complicated, that they rather serve to tire and perplex the observer, than to afford him any real instruction : such are experiments made by electrifying a number of pieces of metal leaves, and making them oscillate at once ; such are also the motions that take place between particles of sawings of wood, which experiment has been described by the Abbè Nollet to the Academy of Paris ; but as this experiment, as well as others
of

of a complicated kind, depend on the principles above described, any particular and minute explanation of them would be unnecessary.

875. I might also, in order to render the repulsion arising from homologous electricities still more manifest, have introduced the instance of a jet of water, produced by the water's own electricity, and also that of such a jet, produced by the presence of an electrified system only. If you present under a conductor, a thin jet of water, which springs upwards, you shall see it divided into a number of small drops; and while those in the middle will rise to the conductor through a very crooked way, the lateral ones will be driven from it.

876. And not only the combined motions of a number of corpuscles at once, but the simple motion of a single corpuscle placed between two systems, will also exhibit considerable deviations and apparent irregularities; and which are such that the usual characteristics and laws of electric oscillations can sometimes scarcely be acknowledged in them.

877. I shall here describe one of these irregularities, which takes place in the apparatus represented in fig. 9. Pl. X. ZP, is a silk hair five feet long; I use it of such length, to that end that the oscillations may be more slow and conspicuous; to this hair a gilt ball is annexed about two lines and an half in diameter, and weighing only two grains. This ball is disposed in the middle between two balls of brass, A, B, which rise from the belly of two bottles, *a*, *b*, and are half an inch in diameter: both bottles lie on the ground; *a* is charged, the other not, and moreover, its lining and coating communicate together.

878. I. When the bottle *a*, has only a small remnant of charge left, the irregularities of the oscillations become still more discernible; the small gilt ball P, from the bottle A, usually rises obliquely, and moves along the oblique line Am, it goes beyond the ball B, and afterwards fall back on it: from the latter it is repercussed with a much less force than it came, and moving through a line Bn—A, straiter than before, turns back to the ball A, without going beyond it. The cause of the lateral deviation of the ball P, lies in the difficulty which this ball P, finds to strike
against

against the two inner points of the balls A and B; in which case it would be directly reflected from the one to the other. In fact, though it be very rare, yet it sometimes happens that the pendulum P is directly carried against these inner points; in such case it moves directly from the one ball to the other, and thus performs two, three, or more oscillations. But if to the balls A and B, two plain parallel metallic surfaces are substituted, then the pendulum P regularly oscillates between them without any irregularity or deviation. To the considerations of both the oblique stroke and reflection that takes place when balls are used, this other must be added, viz. that the pendulum A is repelled from the ball A in proportion to the latter's greater electricity, of which it now partakes: the ball B, which communicates with the ground, is only actuated by the slight degree of deficiency which the ball A can produce in it, by virtue of its own atmosphere; and therefore the pendulum P is repelled by it with a force proportionably less.

879. II. While the bottle *a* continues to be animated with a pretty strong electricity, the whirling motion exhibited by the pendulum P, after it has touched the ball A, becomes extremely varied, according to the numberless combinations that may arise between the different circumstances which may influence the oscillations of this pendulum: such are, I. The particular place of the pendulum on which it is struck by the ball A. II. The angle under which it is repelled. III. The degree of force with which it is repelled. IV. The effect of the weight or gravity of the pendulum. V. The action of the atmosphere of A, which continues to repel that pendulum till it has touched the ball B, &c.

880. Between oscillatory electric motions, and the electric motions produced by *suspension*, there is a great affinity. Out of a silver leaf, I cut a parallelogram B (Pl. V. fig. 6) the angles C, D, of which are very sharp; I suspend it between two metallic plain surfaces, connected with the hooks of two bottles that are both charged, but after a contrary manner; or if the one be not charged, there must be a communication between its coating and its lining. I. When there only remains a very small electricity in the bottles, then the leaf oscillates from the one surface to the other, alternately striking against them. II. While the electricity continues to be
very

very strong, the leaf B remains suspended between the two bottles, only performing very short vibrations, though the distance between the bottles is greater than the intervals to which the points extend their wind; if this distance between the bottles be less than the distance to which the points C, D, can drive the electrified air, then the leaf B remains unmoved. III. If the angles C D, are unequal, then the leaf B remains suspended at distances from each bottle proportioned to the different degrees of sharpness of its angles. IV. If an angle be very sharp, and the other entirely rounded, then the latter turns itself towards the electrified bottle, and remains united to it; as Dr. Franklin has very well observed, while the other angle remains directed outwards, however distant the other bottle may be. When I make this experiment; I usually suspend the leaf with a thin silk hair, in order to discern the better its motions. As for the laws of these kinds of motions, they are constantly the same, so long as the leaves are presented immediately to an electrified system: but it must be observed, that after whatever manner the experiment is made, the leaf, every other circumstances being equal, remains nearest to that system which is not electrified; and the reason of this is, that, as I observed before, the system which is not electrified determines with less force the current of air which transmits the electricity, and makes it spread itself to a less distance than the electrified system.

881. But what else are all the different kinds of electric motions which I have hitherto described, but so many particular consequences of the general law, that the electric fire drives and disposes in its way such deferent bodies as may favour its passage from one system to another? I. Deferent bodies, when they are without any sharp angles, cannot give or receive electric fire but as long as they are in actual contact with other bodies; they are therefore necessitated to oscillate between such bodies or systems as are placed too far for them to touch them at the same time, and alternately to strike against them. II. If the excess of the one system above the other be very small, it cannot then excite any considerable electric wind, and even sharp bodies are, in such cases, obliged to oscillate, and alternately to touch both systems.

III. If

III. If the relative excess be somewhat stronger, so that it may excite some pretty considerable electric wind from the suspended body, though such wind does not extend to both systems, then, the oscillations are shortened, the metallic sheet oscillates through such tracts only as are necessary to enable the electric wind that arises from its sharp angles, to reach both systems, alternately.

V. If the relative excess be considerable, then, as the one of the points of the deferent leaf throws the air deprived of its own natural fire to a great distance while the other point likewise throws the air loaded with excessive fire to a sufficient distance, the leaf remains unmoved.

882. But the law concerning motions of an actual diffusion (9) viz. *that the electric fire disposes deferent bodies in its way, that it may diffuse itself through them with as much celerity as both its own force, and their deference can allow*, this law, I say, becomes particularly manifest, when we consider the arrangement and successive connection of a series of corpuscles lying between two systems: which arrangement of corpuscles, I have usually expressed by the words, *a disposition of the same in the fire's way*. Of this kind of motion I have given several instances, and a number of others might be added. Moist vapours that spring from metallic colipiles contrarily electrified, and the steams of which are obliquely directed relatively to each other, bend themselves towards each other, unite together into one continued stream or series of particles, and by this means the fire which superabounds in the one, is communicated to the other.

883. But with regard to this disposition of deferent bodies in a continued series, this other law must be laid down, viz. *a series of corpuscles is not formed by the electric fire, nor subsists, but as far as this fire finds no easier way to transfuse itself*.

884. The dust of bran which is presented underneath to a conductor, forms conducting strings of its own particles, only when the electricity of the conductor is weak, because it then finds a sufficient passage through such particles as are but little deferent: if the electricity be strengthened, then the strings break; clouds, as it were, of separate particles of bran rise, which by means of their numbers, and of the rapidity of their motions, conduct

duct more fire to the neighbouring body, than the above strings were able to perform.

885. If the intensity is farther increased, then another kind of disposition of bodies *in the fire's way* takes place, which I may call a violent one: the sparkling element then excites most active effluvia from the deferent particles of the water, for instance, which it finds in its way, and by their means explodes the air away; by this means the electric fire most copiously and easily diffuses itself.

886. To the many examples which I have formerly mentioned of violent dispositions of bodies in the fire's way, I shall only add a comparison between it, and another phenomenon of the same kind, which is effected in a milder manner. Make a large drop of water hang from the rounded extremity of a rod of metal, annexed to a conductor: as you will excite the electricity of the conductor, the drop will stretch itself towards the surface of the water that rises from the edge of a glass, full to the brim, placed under it, which it even sometimes attracts; and by successively lengthening and shortening itself, according as the electricity which it endeavours to transfuse will be more or less intense, this drop will exhibit a number of various accidents. But if, through the same drop, a pretty considerable battery be discharged, then the vehement spark will explode the greatest part of it into vapour, and through this vapour, rush united and compact.

C H A P. IV.

On the motion of bodies immersed in an electrified medium.

887. **I**N my seventh letter on Terrestrial Atmospheric Electricity, I wrote very minutely on the electricity which is raised in the air of a room. In the fifteenth letter, p. 343. I gave an account of the electricity introduced into an extensive vaulted room: but it was only after writing the said book, that I perceived that the electricity did not really diffuse itself through the substance itself of the air in the room, but through the vapours only

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with

with which it was impregnated. I at first experimented in a narrow room, where a fire was kept burning in a *Prussian* chimney; but even when there was no fire in the room, I could electrify it, provided I had remained a while in it, with the windows and door shut. In the large vaulted room, I kept a fire in an iron stove, but though the air remained limpid and transparent, yet I was afterwards obliged to acknowledge that the oblique iron pipe adapted to the stove, had not entirely discharged the vapours of the fire, and that to their remnant, though imperceptible, was owing the electricity which I was able to excite. In fact, when I afterwards experimented immediately after shutting the windows, and in very dry weather, I could not by any means, excite any electricity in the air of the room.

883. Thence I had recourse to the expedient of raising, whatever might be the state of the weather, vapours that might favour my experiments. In my little book on *Electric Atmospheres*, which I sent to the Royal Society on the 26th of February, 1769, I related in the numb. 78, how, by burning frankincense, I raised a vapour which very expeditiously and visibly produced the same effects as the vapours which in the night had risen from my body.

889. In experimenting in air thus prepared, I observed, I. That fine flaxen threads annexed to the conductor, soon acquired their greatest divergence; but if I continued to excite the electricity, this divergence lessened very sensibly. This decrease of divergence took place with a kind of regular gradation, especially when a coated sheet of glass was charging: so long as the electric fire, was employed, as it was at first, in forming the charge, it gradually carried the threads to a pretty considerable divergence; but as the electricity grew more intense, then the fire began to diffuse itself out of the sheet, into the deferent vapours spread around it, and it thereby counteracted the electricity of the threads, and soon lessened their divergence.

890. When afterwards the electricity began to be spread to a certain distance in the vaporous air, if I touched the conductor, I observed that the threads annexed to it, vibrated in order to join together; but before they actually effected it, they rapidly moved
again

again from each other. In order more accurately to investigate the cause of these accidents, I excited afresh the electricity of the conductor; then with a flaxen thread very dry, that is little different, I touched the conductor at several successive times; and I observed that, at every touch, the divergence of the threads lessened, so that they at last resumed their natural state: if I then still continued to touch the conductor, a new divergence arose in the threads. I repeated the experiment, and when the threads were brought to diverge in consequence of their own electricity, I directed a metallic point to the conductor, and as I brought it nearer to it, the threads first recovered their natural state, then diverged afresh. When, after the threads had recovered their natural state, whatever might have been the manner of effecting it, I presented my finger to the conductor, I received a spark from it, and a new divergence instantly took place. I charged a coated bottle, supplied with a ball in A (Pl. X. fig. 9); I presented this ball to the threads of the conductor, which were in the state of their first divergence, and they flew from it: I presented the ball to the same threads, when in the state of their second divergence, and they flew to it; and in this state, as I moreover observed, they flew from the belly of the charged bottle which I presented to them, holding it then by the ball A.

891. From the combination of all these facts it follows, I. That in the case of the first divergence, the threads of the conductor did not diverge, but in proportion to the excess of their own fire, above the fire that was diffused in the air. II. When the residuous fire on the surface of the threads was brought to an equality with the fire in the vaporous air, then the threads hung in their natural state: which is the case with all bodies immersed in a medium equally electrified with themselves. III. The two portions of excessive fire, that which was diffused in the conductor, and that in the ambient air, endeavoured not only to destroy each other, but also to drive away the natural fire in them: so that it happened that the conductor drove the fire diffused in the vapours contiguous to it, into continually more remote ones; and reciprocally, when a strange body communicated with the conductor, then the excessive fire that was diffused in the air, began to drive

from the conductor not only the excessive fire in it, but also a proportional part of its natural one.

892. The latter proposition may also be demonstrated by a very simple experiment. With one of your hands, touch the conductor, and present a finger of the other hand to the threads; they will fly from it. Place your finger between both, they will likewise fly from it, increasing their reciprocal divergence: if you present your finger externally to one of them, they will both recede from it: that is to say, the excessive electricity diffused in the air, will drive the natural fire from your finger, as well as from the threads.

893. The nature and effects of the electricity diffused in vaporous air, is rendered conspicuous by the different motions of corpuscles contained in it, which communicate with the ground. Hold two threads between your fingers, and with these walk about the room, after a pretty copious electricity has been diffused into the air of it; and you will see them in any part of the room diverge from each other, as well as from the tables, the chairs, the walls, or any body in the room to which they may be presented: if to the tables, chairs, &c. you have previously annexed threads, you will see the latter diverge, as soon as the electricity begins to diffuse itself, and continue to do so, so long as there remains any.

894. I must not omit here a few observations I have made, with respect, first to the propagation, then to the duration of this electricity. It does not diffuse itself at once from the conductor, but successively reaches parts more and more remote from it; so that if, after only a short exciting of the electricity in the room, the friction is stopped, the threads I hold in my fingers, diverge only at the distance of a few feet from the conductor. In order to have the threads diverge at a greater distance from the conductor, the friction must be continued longer: and in the large vaulted room which I mentioned above, I found that threads, carried to the distance of twenty-four feet from the conductor, did not begin to diverge but after the excitation had been continued ten or twelve minutes.

895. It is of course to be concluded, that the divergence of threads, is less at a great distance from the conductor, than near it.

896.

896. With regard to the duration of the electricity diffused in air, it lasts longer according as the vapours or effluvia through which it is diffused, are rarer and less deferent; therefore, the duration of the electricity is longer in proportion as a longer time has been employed in exciting it. It has often happened to me, when returning into a room from which I had been absent an hour and an half, to find that the threads annexed to a conductor that communicated with the ground, continued to diverge; but then the air in the room was only impregnated with very rare, and as I think, imperfectly deferent vapours, which had escaped out of the stove. At other times, I rendered the air vaporous with two eolipiles; when the electricity raised in it lasted, I found, much less than in the former instance. But when I burnt frankincense, the electricity that was diffused through its smoke, lasted but little: the reason was, that, though the particles of frankincense might be of a pretty much insulating nature, yet they rose very thick, and diffused themselves with such a copiousness as to create in the room a very sensible opacous fog.

897. I shall relate here a very simple method of experimenting on the electricity that takes place in vaporous air, and investigating its laws and different accidents. Let the fig. 3. Pl. V. represent a glass bell C, sufficiently ample, supplied with a rod A B, to which two very thin flaxen threads are annexed; and if the weather be somewhat damp, it will be sufficient to raise a small quantity of vapours under the bell. Place the bell on a small stool; make the hook of the rod A B, communicate with a conductor; and after a short excitation of the electricity, the air under the bell will be competently electrified. If then you touch the hook A, the two threads B will begin to move towards each other, in consequence of their own electricity; but as this electricity will soon cease, they will pass to a second divergence in consequence of the *vaporous* electricity in the room, which will drive the natural fire both from them, and from the rod A B. If I excite again the electricity in the conductor, the threads will again begin to move to each other, in consequence of their own electricity, which is equal to that in the air; but they soon will again diverge, because their electricity will soon prevail over that of the latter, &c. Experiments

periments of this kind may very well, as the reader may see, be made under a glass bell, and by this means, there is no occasion for darkening the whole room with vapours.

898. If after introducing a vaporous electricity into the bell, you proceed to draw the air out of it, very remarkable accidents will take place; but of these, as well as of the electricity of the air and vapours under the bell, I propose to treat in the next chapter.

C H A P. V.

In which electrical motions performed in dilated air, are examined.

899. **I**N the summer of the year 1752, in which I wrote my book on *Natural and Artificial Electricities*, several circumstances hindered me from examining with a sufficient degree of attention, the electrical motions that took place within the vacuum of an air pump. The chief obstacle I met with at that time, was the inconveniency of my air pump, as well as the imperfection of some of the instruments that belonged to it. But having since got an air pump to be made in which the defects of the former were corrected, I shall here relate a few of the observations I have made with it, (Pl. VII. fig. 11, 12, 13).

900. Using jointly with my new air pump, the electric apparatus represented in fig. 9. Pl. II. I made the following experiment. A thin silk hair, four inches and an half long, was fastened with soft wax to the upper part of the inside of a glass bell; to it was suspended the pendulum P, made of tinfoil, weighing rather less than a grain, and distant two lines from the ball B, which received its electricity from the Chain, and only ~~half~~ a line from the ball C, which, through the rod CD, diffused this electricity into the ground: the barometer, at the time of the experiment, was 27 inches, nine lines high (Italian measure), and here follows what I observed.

901. I. When only an inch of air (that is a $\frac{1}{8}$ nearly, of that contained in the bell) remained in it; the small electric pendulum
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very quickly oscillated between the balls B, C, and smartly struck against them. II. When only 9 lines of air remained in the glass bell, the pendulum still moved with great velocity from one ball to the other, so that it was very difficult to count the oscillations; though it must be observed, it then struck against the balls with less force. III. When there remained only about six lines of air in the bell, the pendulum continued to strike against the ball C, but seldom against the ball B. IV. When only 5 lines of air remained in the bell, the pendulum still continued to strike against the ball C, but could only a few times reach the ball B. V. When only four lines of air remained, the pendulum continued to touch the ball C, but reached no longer the ball B. VI. When three lines of air remained, the pendulum now and then reached the ball B, but always kept an whole half line distant from the ball C. VII. When only a line and an half remained, the pendulum still continued to reach the ball B C, but, when falling from it, remained an whole line distant from the ball B.

902. From this experiment it plainly appears, that when the air is much dilated, so that there remain under the bell only nine lines of air, that is $\frac{1}{36}$ part of the atmospheric air; the electric signs seem, at first sight, to have lost nothing of their former quickness.

903. But when afterwards the air becomes dilated to a greater degree, the electric signs under the bell, begin to grow proportionably languid.

904. Yet, even the electric signs that take place in the air, when first reduced to a $\frac{1}{36}$ part of the atmospheric air, are found, if considered with attention, to be less vehement and less rapid, than those performed in air, either condensed, or that retains its natural density. I say, they are less vehement, because in air that retains its usual density, the pendulum, especially if it be somewhat distant from the balls; (Pl. V. fig. 10, 11.) performs around them whirling motions, such as those described in num. 878, 879; the chief cause of which is the intensity and extension of the atmosphere spread around the ball, which communicates with the Chain. When the air begins to be pretty much dilated, so that the fire begins to meet with a free passage, the atmosphere around
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the ball B, becomes less intense and less extended, and the electric motions become of course less complicated; in such case, the pendulum runs from the ball B to the ball C, for instance, through the curve line *y, x, u, t*; it strikes the ball C on its back part *t*, where the atmosphere of the ball becomes less intense, and turns back to the ball B, through the curve line *t, s, r, q*. But even in air that retains its density, such motions may be brought to a great degree of simplicity and regularity. Leave off exciting the electricity, and as soon as it will subside, the pendulum will oscillate from one ball to the other, without any deviation; this is, because the intensity and extension of the electric atmosphere will then lessen together with the electricity of the conductor.

905. But to conclude this subject of motions that take place in air extremely dilated, it remains to be said, in the fourth place, that such motions happen chiefly when the stream of fire is very weak: for instance, I said that when there remains only one line and an half of air in the glass bell, the electric pendulum continues to reach the ball B, and that when it falls back, it remains one line distant from the ball C; now this motion, which is the greatest that takes place in air brought to such a rarity, does not take place when the glass is excited to the utmost, and an uniform, steady, red, violaceous ray takes place between the two brass balls within the bell; it takes place when the friction of the glass, is either very weak, or when it begins or ceases; which fact, however paradoxical it may appear at first sight, is no less conformable to reason than to truth. The electric fire does not set bodies in motion, in consequence of its surrounding them on all sides, and involving them in a continued stream; but it sets them in motion in consequence of its acting with unequal force on opposite sides of them; which is not the case in the above experiment, except when the electric ray or stream, either only reaches the pendulum, and does not yet proceed farther; or when it parts from it, without being succeeded by another electric effluvium.

906. From the same principle may also be understood the reason of another experiment made in dilated air, with minute particles of gold leaf, or other corpuscles of a like kind. On a brass dish (Pl. V. fig. 9) placed on the basin of the air pump, I spread

spread a few such corpuscles : when the vacuum is made, I lower the metallic rod, which is terminated in a kind of ring, so as to bring this ring to a certain distance from the corpuscles : when the electricity begins to be excited, some few corpuscles rise to the ring, but if the electricity is then farther excited, the other corpuscles remain unmoved. The reason is, as I said above, that the beginning effluvium, alone, of electric fire, can move the corpuscles, not a continuous, uniformly diffused stream of it. The motions of these corpuscles, indeed appear very quick, and might make us think at first, that the presence of the air is little necessary to electrical motions ; but a single observation will convince us of the contrary : if the vacuum be brought to a line and an half of air, the motion above will only take place at a very small distance, whereas in the open air, corpuscles move at the distance of three, four, or even six feet from the conductor : from all this it therefore follows, that in proportion as air is more completely dilated, and the electric fire consequently finds a freer passage through it, electric motions lessen, grow languid, and fail. In the experiment of an electric pendulum placed between two balls, at the small distance of about two lines, any small increase of this distance will be sufficient to prevent the pendulum from touching either ball any longer, and make it keep oscillating between both with a great slowness, and through very short arches.

907. In order to obtain as great light as possible on the subject, I have not omitted to experiment in the barometrical vacuum, as I have related in num. 46 of *Terr. Atm. Elec.* The difficulty I found to unite the glass melted with a lamp, around the threads of amianthus I tried to use, obliged me to insert balls made of such threads, into the upper part of the tubes blown into the shape of flasks, (Pl. V. fig. 13.) after having well warmed them on burning charcoal. These tubes I gradually filled with mercury, and successively extracted the air by the help of an iron wire. Even in the best of the barometers I thus made, there must have been a remnant of air ; since when I shook it in the dark, I could perceive a short ring of electric light* which accompanied the circum-

* I mean to speak of the little lucid ring produced by friction, which I have analysed at the end of the chap. II. that the light of *communication* may appear in a complete

ference of the mercurial cylinder; which luminous appearance I could not perceive in other barometers more completely emptied of air, which I succeeded to make when I inserted no threads of amianthus into them. With the barometer made as described above, I experimented as follows. I. Into a tube of glass exactly equal and similar to the tube of the barometer I used, I likewise inserted a ball of threads of amianthus, which I made communicate with the ground through an iron wire; the mercury of the vessel into which the tube of the barometer was dipped, communicated also with the ground. Making choice of a very dry day, I insulated myself, and communicated with the Chain. When I presented my finger to the top of the barometer, a small spark leaped to it, and in the same instant the whole empty space of the barometer, (*Terr. Atm. Elec.* p. 48) was filled with an electric light uniformly diffused, of a red purpureous colour, which was less vivid than the light of the spark, in proportion as it was more extensively diffused: but while this took place, I could hardly perceive a motion in the threads of amianthus. On the other hand, when I presented after the same manner, my finger to the other tube, made of the same shape as the barometer, a spark no sooner leaped from my finger to its external surface, than another leaped from the internal one, to the threads of amianthus; which spark often shook not only a few of the threads, but the whole aggregation of them. In short, I ascertained this truth, that even in very minute bodies, the electric motions grow very weak in proportion as the vacuum grows exact in which the experiment is made. I continued to experiment, and made bubbles of air enter into the barometer, and then observing in the dark, I saw them lighten as they were rising: some of them were not unlike those lightning globes which sometimes are seen during *Auroræ Boreales*; but the circumstance I chiefly propose to observe, is that the mercury in the barometer was scarcely lowered three inches, when the whole mass of threads began to be agitated at every spark that was drawn.

vacuum, is what I never doubted, and I have even described a number of instances of such light.

908. Though I have hitherto only mentioned the vibrations and complicated motions of threads of amianthus; yet I have also examined in the vacuum motions of a still simpler kind: here follow the last experiments I have made on that subject. I. In a glass bell nine inches high, four wide, and through the neck of which a brass rod was inserted, I suspended two fine threads about sixteen lines long, to the extremity of this rod, with balls of pith of elder hanging by them, and so light that they scarcely could stretch them. II. I established a communication between the rod and the conductor, and after the glass had been rubbed a long while, there arose within the glass bell an electricity that made the threads begin to move towards each other when the conductor was touched; but they soon passed to their second divergence, resulting from an *ambient* electricity: if the conductor was again electrified, the threads again began to move towards each other, and afterwards diverged in consequence of their own electricity. III. I then began to make a vacuum under the glass bell, and after the first strokes of the embolus, when the usual fogginess began to appear within the top of the bell, it seemed as if the divergence of the threads in consequence of the electricity in the ambient air, were somewhat increased; but this increase was but small, and lasted but little: when I drew more air, the divergence proved still less. However, though the air was afterwards almost completely extracted, so that little more than two lines of it remained within the bell, yet a divergence continued to take place between the threads, no less than fifteen or twenty degrees. IV. Having farther dilated the air, so that the remnant of air was now no more than two lines, I saw the electric fire flow from the threads to the basin underneath, under the appearance of two continued rays: when these rays flowed with some intensity, the threads immediately fell into a contact with each other, but resumed some divergence whenever the rays seemed either to cease or grow languid. V. Admitting afterwards the air under the glass bell, and mean while touching the Chain, I perceived the divergence of the threads increase; but it soon began to lessen very sensibly. Though the air was now freely admitted into the bell, yet there was still a small remnant of divergence; and when after raising the bell from the

bason, I presented a brass rod to the threads, they receded to a certain short distance from it. VI. The above accidents are to be observed, when the balls are placed pretty high in the vacuum, for instance, at about five inches above the bason: if the rod be lowered, then the balls, as they move downwards, lose their divergence, so that, when they are only an inch distant from the bason, they diverge no more, however strongly the electricity may be excited, and the fire is then seen continually flowing through the vacuum without moving them.

909. The latter circumstance seems to indicate that the electricity by virtue of which the balls diverged, arose from some fire, which from the metallic neck of the bell, ran along the external surface of the latter, and actuated an electric atmosphere from the corresponding internal surface; because the experiment was made in good weather, and the bell was smooth and free from vapours. However, when can air be said to be completely pure? I. It might be, that the lower vapours retained little or no electricity in consequence of their being nearer the deferent bason. II. The increase of the divergence between the two balls of pith of elder, that took place when the air began to be drawing out, may have been owing to the vapours that gathered near the top of the bell, where they formed a kind of fog, and thus encreased their own electricity, after the same manner as we have seen that a man increases his own electricity by gathering his limbs together on an insulating stool, (num. 457). III. The little increase of divergence that took place when the air began to be admitted, was perhaps owing to a like cause; in that instant the electrified vapours were driven towards the top of the glass-bell. IV. In repeating this experiment, I have several times, both before extracting the air, and after, wiped the bell with a handkerchief, and the divergence still continued. V. The same divergence happened several times to continue, though at the same time a very fine thread which I presented externally to the bell, was not attracted.

910. But whatever the cause of this electricity might be, whether it arose from the fire that flowed externally, or internally, along the surface of the glass, or from the fire that actuated the vapours diffused within the bell, and after drawing off the air, still

still continued in them, though they became much rarer, this at least is certain, that the effects of this electricity were entirely similar to those that obtain in the *aereo-vaporous* electricity: and in the second place, from these experiments it seems to result that the natural quantity of electric fire lies diffused in the vacuum, in the same manner as it does in other mediums or bodies; in fact, both the excessive fire which is actually accumulated in the vapours of the air, brought to the rarity of two lines, and the excessive tension which takes place in the fire of such a medium, suppose the presence of the natural quantity of it; nor does it seem that this natural quantity may be actually diffused in so rare a medium, without being likewise diffused in more complete vacuums. And here we must add, in the third place, that air resists the electric fire, keeps it back, and compells it to accumulate, by making it coalesce, not with the substance of other electric fire contained in it, but with its own substance. In the fourth place, with respect to our present enquiry concerning the mutual electric recessions of bodies in dilated air, it appears that they are proportioned to the quantity of the electricity that is retained by the remaining air.

C H A P. VI.

The causes of electric motions.

911. **W**E are under no necessity of explaining electric motions by certain indeterminate kinds of attractions, or repulsions, as Sir Isaac Newton has been obliged to do with respect to the phenomena of the universal gravity. No mechanic principle of gravity has yet been discovered by our senses; and all those which men have hitherto imagined in order to explain this phenomenon, have been found insufficient, nay repugnant to the evidence of facts: but in regard to electric motions, we manifestly perceive them to be owing to a most active element, to the *inequilibration* of which electric motions exactly correspond, with respect both

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to their existence, and to their quantity and quality. I say with respect to their *existence*, because electric motions never take place but when the electric fire is unbalanced, and actually endeavours to recover its equilibrium: I also say, with respect to their *quantity*, because they are always found to be proportioned to the greatness either of such unbalancement, or of the diffusion of it that takes place; and also to their *quality*, because the same kinds of motion are always seen to take place between bodies contrarily electrified, and likewise between bodies similarly electrified. If to these considerations we add that of the electric atmospheres, that is to say, if we reflect that from the unbalancement of the electric fire in deferent bodies, an adequate unbalancement always arise in the fire belonging to the ambient air, whence arise motions of *simple pression* in those intervals through which the electric fire cannot actually flow, and sometimes also of *actual diffusion*, it will, I say, appear evident *that between the actual unbalancement of the electric fire, or a diffusion of the same, and the motions of bodies, there is a connection of both causes and effects.*

912. The discovery of such a connection invites us to seek for some more precise operation or action of the electric fire, by which, when it endeavours to recover its equilibrium, it produces electric motions. Not that I think we may presume to discuss the nature of the first elements of the electric fire, and of its primitive forces; all these are beyond the reach of our senses, consequently beyond the sphere of our understanding. We are to look for the principles on which we mean to ground our theory, in the last phenomena to which our sensitive faculty can extend.

913. Now the last phenomena to which my sight, my feeling, and in general my senses have allowed me to reach, have all of them confirmed this truth to me. I. That, *the electric fire, neither in insulating nor in deferent bodies, can either be dilated by drawing it to contrary parts, or condensated by trying to compress it from opposed parts.* This first truth, as far as concerns glass, is conformable to the Franklinian theory, and is demonstrated in a very simple manner by the experiment related at p. 64 of the *vindicating electricity*; which consists in touching both coatings of a plate, with
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the hooks and the coatings of two bottles equally and similarly electrified; when the coatings of the plate are found to have contracted no electricity. With regard to such other solid bodies, as may be electrified in the same manner as glass, the like result will be obtained *. With respect both to insulating and deferent bodies of a fluid nature, the same is manifest from the experiment of the electric *well*; in fact, the natural state either of the fire of the cavity of the *well*, or of the bucket suspended in its middle, or of the air contained in it, are not in the least altered, whatever electricity may be introduced into the substance of the well.

914. *The electric fire can be condensated or dilated on alternate surfaces, or strata, of insulating bodies, provided at the same time it is condensated or dilated on the other alternate surfaces.* This second truth, is the same as the Franklinian principle concerning charges of sheets of glasses, or insulating strata of any kind. Fire can be condensated on the internal surfaces of a series of bottles, A B, C D, when the hooks of every one communicate with the coating of that which stands next; so that the fire may be dilated at the same time on all their external surfaces. The same we have seen to take place, with regard to strata of insulating substances: for instance, from the interior surface of the *well*, a certain quantity of fire may be driven away, and condensated in the air contiguous to it, provided an adequate portion may be likewise driven from that surface of air, which is contiguous to what we called the *soul* of the *well*.

915. III. *Within the substance of deferent bodies, the electric fire cannot be either dilated, or condensated, in any permanent manner.* This third truth is manifest from the experiment formerly related,

* We are not to object against the principles laid down above, that when a glass plate is rubbed on both its surfaces jointly, there results on both these surfaces, homologous electricities, and that the same takes place on a silk ribbon. The act of rubbing either lessens or increases the capacity of the rubbed body, as I have shewn at the end of the chap. III. It is not therefore surprising, that both surfaces in such case, should either give, or receive fire; but as soon as both recover their natural capacity, they immediately endeavour either to throw out fire, or draw the same, till they have recovered their former natural quantity: from all this results the intense atmospheres that take place around insulating bodies, when rubbed.

of the electric *well*: the bucket when extracted from the bottom of it, brought no electricity along with it; now, if any electricity had been condensed within this bottom, the bucket would have manifested some sign of it; but to say it once more, electric fire can never be condensed without a correspondent dilatation of the same. Hence, the inalterability of the natural quantity of electric fire in deferent bodies, which at first appeared to us to be a kind of inexplicable mystery, is found to be a consequence of an universal propriety of this fire, viz. of refusing, in any medium or body whatever, to be either compressed or dilated; unless it be in certain particular cases, when alternate condensations and dilatations of it take place in the same directions: which alterations cannot take place in deferent bodies, in consequence of their very deferency*.

916. IV. Over the surface of deferent bodies, electric fire may be permanently condensed or dilated; but this is only when a *correspondent quality of natural fire may alternately be condensed or dilated through successive strata of the air, contiguous to them.* This proposition constitutes the principles on which the theory of electric atmospheres, explained in Section III. is grounded, and is a consequence of the experiments on the electric well, which I have just quoted. On the surface of the body E (Pl. VIII. fig. I) fire may be permanently condensed on the concavity of the successive strata of contiguous air, correspondently to the fire that may be dilated on the convexity of the same, &c.

* I confine this proposition to the case of *permanent* condensations, arising from the contrary actions of simple atmospheres; though, in the case of a spark that runs into the substance of a deferent body, or out of it, there is also a kind of transient condensation of the fire; which is manifest from the effects produced by such sparks: but then these condensations proceed from forces that all conspire to produce them in a certain sole direction; besides, these condensations do not take place within the deferent substance, otherwise than correspondently to the alternate condensations and dilatations which they may create in the natural fire of the ambient air, against both the air that surrounds the wire, and that which is contiguous to the ground: whence it happens, that those who are distant even fifty paces from the place which is struck, feel a great shock, especially in their legs; this I propose to treat more minutely in another book in which I shall deduce from observation, the more exact and precise precautions that are to be used for conducting lightning.

917. V. *The natural fire of the air which is situated in the middle between two bodies similarly electrified, cannot be alternately condensed or dilated, but in oblique directions outwards, as I have explained when treating of the electricity which is exerted from the upper cavity of the well.*

918. VI. *It therefore follows, that neither the excess nor deficiency of two bodies electrified alike, can be accumulated on the internal surfaces* of these bodies, but are compelled to flow to the external surfaces of the same.* If two bodies, E and e (Pl. VIII. fig. 5.) are electrified by excess, this excess will endeavour, from opposite parts, to compress the natural fire of the air lying between them; which will prevent an alternate succession of dilatations and condensations from taking place: now, as fire cannot be condensed on the surface of bodies, but as far as there is a possibility for the above alternate succession of dilatations and condensations, the consequence will be that on the *internal* surfaces of bodies, no fire can be accumulated, but will be compelled to condensate itself on their external surfaces, that is, on those places where the condensating forces do not counteract each other. It may be proved after the same manner, that on the *internal* surfaces of bodies no dilatation of the natural fire can take place, and can only arise on their external surfaces.

919. VII. *The excess and the deficiency of two bodies contrarily electrified, must be accumulated on their internal surfaces.* Because there the excess in E, and the deficiency in D (Pl. VIII. fig. 7) endeavour, in the same direction, to produce alternate condensations and dilatations of the fire in the air between these bodies; that is, the excess in E, as it accumulates itself against the air internally contiguous to it, endeavours to drive the natural fire from the air contiguous to D; and reciprocally, the dilatation of the fire in the air internally contiguous to D, endeavours to draw the accumulating fire from the body E, against the air internally contiguous to the same: now, as no such co-operating actions as these take place externally, both the excess in E, and the deficiency in D, will be accumulated

* By the word *internal surfaces*, the author means those which face each other.

on their internal surfaces. To express the same effects otherwise, I might say, the excess in E is a force that internally drives the fire remaining in D; and the deficiency in D is like a support that is taken from the excess in E.

920. From all the propositions above, the following simple principle is to be deduced: *all motions of electric bodies are effected from the places of the greatest reactions of their atmospheres, to those where such reactions are least, and such motions are besides proportioned to the differences of these reactions; or in other words, all electric motions arise from places in which the alternate condensations and dilatations of the electric fire cannot be effected, or are effected with a great difficulty, to those where they can be effected with greater ease.*

921. With regard to bodies positively electrified, the case is manifest; they reciprocally recede from each other: that is, they move from the middle or internal space between them, in which the alternate condensations and dilatations on the one surface, cannot correspond to those that arise on the other, and where consequently the excessive fire is reciprocally repelled from these internal surfaces, and move towards opposite external parts of the ambient air, where they can easily affect alternate dilatations and condensations, and where excesses of fire can be accumulated against the contiguous air: that is, from the place where the reaction is greatest, they move towards the place where it is least.

922. The same is also manifest with regard to bodies negatively electrified. From the middle interval, in which alternate condensations and dilatations cannot arise in consequence of the dilatation that takes place on both the internal surfaces of such bodies, and where therefore the remaining natural fire is reciprocally attracted to these internal surfaces, the bodies move towards opposite external parts of the air, where they can effect alternate dilatations and condensations, and where consequently deficiencies of fire can be accumulated, correspondently to the fire that may be relaxed towards the external surfaces of the contiguous air.

923. With regard to bodies contrarily electrified, that which is positively electrified moves towards that which is negatively electrified, where a deficiency favours those alternate condensations
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and dilatations which the former endeavours to introduce into the natural fire of the contiguous air: and reciprocally, the body negatively electrified, moves towards that which is positively electrified, where an excess favours the alternate dilatations and condensations which the former endeavours to introduce into the natural fire of the contiguous air.

924. And thus, since the production of alternate condensations and dilatations, always depends on the state of the two bodies, it follows, that *approaches* and *recessions* of electrified bodies are always reciprocal.

925. In the fourth place, if a body E (Pl. VIII. fig. 3.) be positively electrified, and the body N be not electrified at all, the alternate condensations and dilatations will be equally on all sides around the body E; because the excessive tension which will be introduced by the excess in E into the air externally contiguous to the body N, will be equivalent to the excessive tension which the excess in E introduces elsewhere into the air internally contiguous to the same body N; which according to the hypothesis, retains its whole natural quantity of fire, and no more; therefore this body N will be balanced between excessive tensions that will be equal on all sides. The body E will be likewise balanced between excessive equal tensions, viz. between the tension which the excess of E introduces into the air in the middle, and the reaction of the equal tension which the whole fire of N will introduce into the fire of the external air †.

926. In the same manner, two bodies, the one D negatively electrified, and the other N, not electrified, (Pl. VIII. fig. 4.) will both remain balanced in the fire of the air around them, which is every where equally relaxed: the reason is, because the relaxa-

† Universally, the reaction which arises from the fire repelled to the external surface of a body N is less, according as this surface is ampler, and spreads a tension into ampler strata of air: under such limitation we are to understand this paragraph and the next. The more the external surface of N will surpass the internal surface of it, the less the reaction against the external surface will be; and the more imperfectly the equilibrium will subsist.

tion of the fire in the external air, relatively to N, will be equivalent to the relaxation which, from the deficiency of D, will arise, in some other place, in the fire of the air which is immediately and successively contiguous to the body D; and the relaxation of the fire of the same external air, (since N is supposed to retain its natural quantity of fire) will be equivalent to the relaxation of the internal air.

927. Here we have therefore, if I do not mistake, brought electric motions to an *unity* together, and explained the efficient cause of them, without having recourse to two distinct currents of electric fire; a supposition not only superfluous, but totally repugnant to the evidence of facts.

928. The cause above assigned to electrical motions, besides its conformity to phenomena, has this farther advantage to recommend it, that it unites and proves to be the same with the efficient cause of sparks. The reason why the fire which is redundant in a system leaps, condensed into one spark, towards another system which is relatively deficient, is nothing more than the excess of reaction which the fire of the ambient air exercises in some place against the fire of that system, above the reaction exercised against the same fire, by the fire of the air between the two systems. And indeed it is very natural, that as electric signs are subject to the same laws, they should arise from the same causes.

929. Is it not besides this same reaction which, by being in some particular place increased, drives the spark into a given direction, that is the cause why, in the instant this spark takes place, that it is in the instant of an actual diffusion of the fire, the velocity is increased with which bodies contrarily electrified move towards each other. In fact, if the mere relaxation of the natural fire in the air between two bodies, produces their mutual approaches of *simple pression*, these same motions must be more quickly performed as the relaxation increases in consequence of the bodies being brought nearer to each other: this motion must likewise be more rapid when the relaxation of the natural fire in the

the air between the two bodies, is turned into an actual motion, when the remaining resistance actually yields to a reaction which is somewhere else superior, and when the electric fire relatively redundant around a system, drives from the substance of it an equal portion of fire into the substance of another system which is relatively deficient. These positions are entirely consentaneous to the universal principle, viz. *that any solid body, or fluid, whatever, presses against adjacent substances with a part of the force by which it is actuated, that is less in proportion as it employs a greater share of this force in its own actual motion* *.

930. If the principles above are properly applied, they may also with the greatest clearness explain other more complicated electric motions. Certainly, we may dispense with saying any thing about electric oscillations; since they are formed by the mutual approaches of bodies contrarily electrified, and the mutual recession of the same, when become similarly electrified by virtue of their mutual contacts; only, such vibrating motions usually take place in that body alone which is placed in the middle, in consequence of the immobility of those placed at the extremities.

931. The above oscillations are turned into mere *suspensions*, when the body finds itself in the middle between equal active parts of the same current. The little pendulum suspended in dilated air, as we saw before, remains suspended in the middle of the rare continued ray, and does not vibrate through short oscillations but in the instant when the current by which this ray is formed, begins or ends, and consequently prevails on either side of the pendulum. In the same manner, a pendulum of a considerably greater weight than the former, remains suspended in the middle of the fulgid, continued, and loud stream of fire, which sometimes runs between the two bells with which I explore the state of the atmospheric electricity: it seems that, as

* The words of the original are the following,—*Che un corpo o fluido qualunque, o solido, con tanto minore porzione della forza qualunque, da che si animato, preme le aggiacenti sostanze, quanto è maggiore la porzione di quella forza, che impiega nell'attuale movimento sua.*

the former thin ray suppresses the reaction of the rare remnant of air in the bell, so the dense stream of fire in the latter case, also suppresses the strong reaction exercised against the heavy pendulum by the dense atmospheric air.

932. The manner after which a gold leaf remains suspended, seems to confirm the above explanation: it seems that the distension of it must be imputed to an excess of the reaction exercised on its plain surfaces, above that exercised on its sides and angles, which find themselves in the middle of a continued stream. Is not the reaction of the electric wind, which in those short instants when it prevails over the electric reactions which decrease in consequence of the fire that actually diffuses itself; is not, I say, this reaction the cause why the gold leaf thus circumstanced slips away?

933. But in the motions of a *disposition or direction of bodies into the fire's way*, I think I still better discover the reality of the cause which I am assigning here. When I see a suspended drop of water, lengthen itself towards the water that is presented underneath it in a deferent vessel, and mean while see the latter rise towards the drop under the shape of a little hill, I cannot help thinking but that the fire which from the lower part of the drop diffuses itself into the water and thence into the ground, does not exercise the same degree of action and reaction on both this drop and the water, which the natural fire in the adjacent air exercises in the space around that same drop, and upon the even surface of the water, around the little hill raised above it; because, in the same manner as the natural fire in the air contiguous to a body, suffices to drive the natural fire out of that part of it which is immersed, for instance, in the atmosphere of the Machine, so I think that the natural fire of the air contiguous to the metallic leaf, suffices to render its surfaces even, and distended, in consequence of the less reaction exercised on its points, through which the fire actually flows. And thus also I think that the natural fire of the air contiguous to the water, suffices to raise it towards that place where the fire actually flows, and of course exercises a less reaction.

934. Again, when I see particles of bran rise from a metallic basin, and unite together into rows or strings, and the latter remain suspended between the conductor and the basin, in order to conduct the subtle continued stream of fire, I think that this obtains, because the particles of bran, in the place of their mutual contact, where the fire freely flows, meet with a less reaction than in any other place externally, where the natural fire being affixed to the air, exercises a considerable reaction; or even in any other place internally, where, for instance on the basin or the conductor, the fire is more diffused and divided.

935. The transformation of mutual approaches into mutual recessions, confirms this idea to me. Since this transformation takes place when the electric fire, which, in order freely to pass from a body into another makes both move to each other, is at last retarded, and obliged to accumulate itself so as to have its reaction much increased, it is natural that when the stream of fire is increased beyond the quantity which the particles of bran are capable of conducting, the strings made of such particles should break, and these particles separate.

936. Conformably to the same principle, mutual approaches are likewise transformed into mutual recessions, when the traversing electric fire excites either in the bodies, or in the medium around them, such alteration as may cause the action arising from it, to surpass the decreasing reaction which results from the passage of the fire: as for instance, when spark *a* drives moisture into vapours intensely repulsive, or destroys that part of the body out of which it rushes, or into which it runs, and sometimes drives it into vapour: in both these cases it explodes the bodies towards opposite parts.

937. And lastly, the experiment of the *aereo-vaporous* electricity, which has been lately described, also confirms the theory of the reaction of electric atmospheres, and their efficiency in producing electric motions. In fact, all the experiments that I have related with regard to vaporous air positively electrified, equally succeed when air is negatively electrified. And as the natural fire of bodies is balanced by the natural fire in the air,

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so the natural fire, whether increased or diminished in vaporous air, is balanced by the natural fire of those same bodies, which is likewise increased or diminished. As the excessive fire of bodies is accumulated on their surfaces, and creates a particular tension in the natural fire of the air over it, when unmixed, so the natural fire of bodies accumulates itself on their surfaces, against the diminished and relaxed fire of vaporous air. As the natural fire of unmixed air exerts itself against the fire that is relaxed on the surface of bodies, and drives it away, so the fire increased in vaporous air drives the natural fire from the surface of bodies, &c. Here follow a few differences between the above two cases.

I. The unbalancement between the fire of bodies, and that of unmixed air, arises only from an alteration in the quantity of the natural fire of these bodies; but the unbalancement between the natural fire of bodies, and the fire of vaporous air, may arise from an unequality in the alteration of the quantity of fire in the vaporous air, and in those bodies. II. The latter unbalancement is of less duration than the former, in proportion as the unbalanced fire may more easily diffuse itself through the vaporous air, than through unmixed air.

938. If I could flatter myself that I have satisfactorily explained all the above phenomena, I would venture to express my satisfaction, by concluding this chapter with the experiment of an electric dance, which occurred to me a few years ago. At all events, I will subjoin the experiment here; I suspend two long very fine flaxen threads, to the lower surface of the conductor: I wet them and keep them separated from each other, till the electricity is grown somewhat intense; then leaving both at liberty they begin to dance, incessantly vibrating their extremities, and continue twisting and untwisting each other, in contrary directions, alternately: that is to say, from the interval between them, they throw their extremities outwards, and throw a moist effluvium equally electrified with themselves, into the ambient space; and as their electricity is thus diffused within a certain plane, they are repelled in some other plane which has received no such electricity.

Second

Second part of the Sixth Section.—On the electric motions of insulating bodies, and on the *vindicating* electricity.

C H A P. I.

On the electric motions, and the vindicating electricity of insulating bodies of a rare texture, consequently incapable of a charge.

939. **T**H E laws concerning the electric motions of deferent bodies, are also applicable to insulating bodies. I. Two insulating bodies similarly electrified, move from each other. II. Two insulating bodies contrarily electrified move towards each other. III. Between an insulating body that is electrified, and another which is not, no motion takes place.

940. The same law takes place likewise between a deferent, and an insulating body. I. An insulating body, and a deferent body, similarly electrified, move from each other. II. An insulating body, and a deferent body, contrarily electrified, move to each other. III. And if of two bodies, the one insulating, the other deferent, the one be electrified, the other not, and no diffusion of electricity can take place between them, no motion will arise between them.

941. But here we must observe two very remarkable differences. I. Two insulating bodies, or an insulating and a deferent body, contrarily electrified, after joining together remain strongly united; in which they differ from deferent bodies, which having in their mutual contact equalized their respective quantities of fire, repel each other in proportion to the fire that remains in both; or part without showing any farther electric sign, if their excess and deficiency, being at first equal, have reciprocally annihilated each other. II. After an insulating body has joined with another insulating

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lating body, or with a deferent body, and they have lost their contrary electricities (so far as these were equal) the insulating body, as soon as it is taken from the other, *recovers* its former electricity: to this property I have reduced the many various phenomena which have been observed, partly by others, and partly by me, with experiments on several kinds of insulating bodies. It is this property which I have expressed by the words, *vindicating* electricity.

942. I think I have been the first (as Dr. Priestly observed in p. 271 of his history) who gave instances of the motions of *insulating* bodies, of their mutual adhesion, and of their *vindicating* electricity. The instance which I produced in the 196 p. of *Natur. Elec.* I had been supplied with by Signor Alexandro Vaudania, whom I had desired accurately to observe the electric phenomena or accidents, which happened to him when he undressed himself; and who, in consequence, sent to me the following account. "Since the cold is grown sharper, that is, for these ten or twelve days past, I have put on, between my two linen shirts, a shirt or under-garment of beaver. Every night, when I take off my upper shirt, which I change every day, I perceive that it has a kind of adhesion with the beaver garment; and when I separate them I hear a number of cracks, produced by sparks; these, when observed in the dark, do not differ from those which take place in electric experiments. When I afterwards take off the beaver under garment, I feel it sticks still more to my under shirt: after forcibly separating them, I perceive that the lower edge of the beaver continues to adhere to the shirt, and draws it off from my body: having entirely separated it from the shirt, the latter flies to my body; I then approach again the beaver, and the shirt flies to join it again; I separate them, and the shirt flies to my body." In these facts we manifestly see the insulating beaver adhere to the linen shirt and lose its electricity, and then, by virtue of a forcible separation from it, recover it; whence it is afresh enabled to join with the shirt, to draw it off, &c.

943. Signor Symmer afterwards promoted much the same kind of experiments, being led into it by the sparks which he heard and saw, when drawing his silk stockings at night. After many

many trials he found that the electric phenomena became particularly intense when he put on a white silk stocking, and upon this another of black silk: he observed that as long as these were upon his leg, they gave, though rubbed ever so strongly, but very weak signs of electricity: when they were drawn off together, they hardly gave any; but when he afterwards drew the white stocking from the black one, most vivid signs of electricity arose, by excess in the white stocking, by deficiency in the black one. Being kept at some considerable distance from each other, they remained both swelled as if his leg had been still within them: bringing them nearer to each other, the swelling was gradually diminished, their mutual attraction increased, and they at last precipitated themselves towards each other with an amazing violence, and closely united: being again disjoined, they exhibited the same signs of electricity, the same phenomena. When the stockings were kept separated, they quickly lost their electricity, as does a rubbed tube; but when they were left joined, they preserved it for two hours, or even more. Nor was it possible, when they were in such a state to give them, or to draw from them, any electric fire, not even with the sharpest point: their union at the same time was so strong, that a weight of seventeen ounces suspended to one of them could not separate them: nay, when the black stocking was fresh from the dying, and the white one recently whitened with smoke of brimstone, they resisted to a weight of three pounds three ounces, &c. When, to two stockings thus sticking to each other, the same gentleman presented two others more strongly electrified, viz. a black one to the white one, and a white one to the black one, the former immediately parted and united with the latter. When the stockings thus presented were electrified in a less degree than the former, the former remained united, their adhesion was only weakened by the presence of the latter, and they all weakly united together into one mass. The same Signor Symmer also observed, that not only electrified stockings united together, but also remained suspended to deferent bodies.

944. The Abbé Nollet in the third volume of his Letters, has made several observations on the experiments of Signor Symmer;

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he rubbed stockings stretched on a deferent plane, and afterwards transferred the experiments from stockings to ribbons, which he electrified after several different manners. Signor Cigna, physician, in the third volume of the Royal Society of Turin, has carried farther the experiments of Signor Symmer, and of the Abbé Nollet, with silk ribbons, of which he combined the number and colour after different manners: he besides rubbed them on planes insulated, deferent, smooth, rough, hairy, &c.

945. In order to treat this very complicated matter with as much clearness as possible, I think we must begin with observing what kind of electricity is introduced into a given insulating body of a rare texture, in consequence of a friction made against it, when suspended in the air, by another given body. With regard to this I have observed, I. That by keeping a ribbon stretched in the air, and rubbing a side of it with a given body, there arises in it a weaker electricity, than if both its sides were rubbed at once. II. When the ribbon thus suspended was rubbed on both its sides at once with different bodies, the one of which was apt to create an electricity by excess, the other an electricity by deficiency, there resulted in it an electricity proportioned to the difference between the intensities of the contrary electricities that would have resulted from separately rubbing the two sides of the ribbon, first with the one of the two above bodies, and then with the other.

946. I have afterwards found that if an insulating body A, of a rare texture, be stretched on a plain body B, and be rubbed upon it with another body C, the electricity that rises in A, is proportioned to the sum of the similar electricities, or to the difference of the contrary electricities, which are introduced by the friction of C, and the *counterfriction* of B. That is to say, I suppose that the body A which is rubbed, has its parts so flexible that the body C, by moving along the one surface of the said body, and all the way pressing upon it, communicates a nearly equal motion to the other surface of it which is stretched on the plane B: from this pressing progression of A on B, arises what I called the *counterfriction* of B against A; this moveableness or flexibility, I have found to take place, not only in stockings, but also in several compact kinds of silk stuffs, for instance in sattin. Therefore, if
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with ivory, with iron, with brass, with copper, I rub a white ribbon on a plane made of any one of those same bodies, as they all draw fire from the ribbon, the latter will become negatively electrified. Likewise, if I stretch the same white ribbon on a board of walnut-tree, of beech-tree, or on a black silk stocking, and rub it with a piece of any one of these bodies, as they all give fire to the black ribbon when suspended in the air, the latter will become positively electrified. If with one of such bodies as give fire, I rub the ribbon on one of such bodies as receive it, then no electricity takes place, or at least a very weak one, the nature of which is very often uncertain; a circumstance which in several experiments has perplexed me: but at last this electricity prevails which is most active; and this activity may be either increased or diminished by several accidents. Here follows an instance of it: a little blade of gold always electrifies by excess a white ribbon stretched in air, either when the friction is made with the plain surface of the blade, or when it is made with the angular side of it*. Likewise, if I stretch a white ribbon on a plate of glass, and rub it there with the plain surface of the blade, so that thence arises no counterfriction from the glass, or at least a very small one, the ribbon also acquires a small electricity by excess: but if I rub the ribbon with the angular side of the blade, so that by inserting itself, as it were, into the furrows of the ribbon, it thus produces a strong counterfriction from the glass, there arises in the ribbon a negative electricity, proportioned to the excess of force which the glass has of drawing off the fire of the ribbon, above the force which the gold has of supplying the same.

947. To the preceding rule which is laid down with regard to a body rubbed when stretched on a plane, this other is entirely conformable, which has for its object the electricity that results within two flexible substances, placed together upon a plane. If,

* I have represented in the fig. 10. of the Pl. X. the instruments which I use for rubbing ribbons; they consist of a wooden handle, and of a blade of iron or of steel, when I mean to rub with iron, or with steel: but this same blade is lined underneath with a thin plate of gold, or of silver, &c. when I propose to rub with either of these metals.

for instance, two white ribbons are placed on each other, then stretched on an insulating plane, and the upper one is rubbed with any body whatever, it will be found that when the two ribbons are, after the friction, taken from the plane, either jointly, or the one after the other, they will repel each other, and manifest homogeneous electricities, which will be either positive or negative, according as both the rubbing body, and the contra-rubbing plane are apt to give, or to draw off, fire; or they attract each other, and manifest contrary electricities, if the rubbing body is apt to give, and the *counter-rubbing* plan to *draw off, fire, and vice versa*: that is to say, the upper ribbon in all cases manifests the same electricity which it would manifest, if, being alone and stretched in the air, it were rubbed by the above body; and the under ribbon manifests the same electricity it would, if, being alone and stretched in the air, it were rubbed by the *counter-rubbing* plane. Thus, if I stretch two white ribbons on a plate of glass, and rub them there with ivory, with iron, with copper, with brass, &c. as the latter bodies receive in this case, fire from the ribbon which they rub, and the counter-rubbing glass also draws fire from the other ribbon, both will become negatively electric; but if I stretch the two ribbons on a plate of sealing wax, or sulphur, and there rub them with sealing wax, or with sulphur, or with a black stocking, they will both become positively electric. Lastly, if the ribbons being stretched upon any one of the latter bodies, which are apt to give fire, and rubbed with any one of the former, which are apt to draw fire, or (*vice versa*) the ribbons will manifest contrary electricities.

948. V. If the plane on which the similar ribbons are rubbed, be deferent, or though being of an insulating nature, does not actually completely insulate, either because it is not polished enough, or because the ribbons themselves are imperfectly dry, then the law laid down in the preceding paragraph, is proportionably disturbed. The reason of this will soon be understood if we consider that any electricity endeavours to turn an homologous one into a contrary one, if the latter be less intense, and is enabled to diffuse itself away: it therefore may easily happen that the electricity of the upper ribbon will turn that in the under ribbon, into a
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contrary one, whenever the fire in this under-ribbon will be able freely to diffuse itself through the plane. To this case may be reduced all the alterations that may take place in the preceding rule.

949. If two flexible bodies possess different electric properties: if the one, for instance, be a white, and the other a black ribbon, then the electricities that will result in them, will be conformable to the electricity which the rubbing body will introduce into the upper ribbon, and to the electricity which the plane will introduce into the under-ribbon; and besides, to the alterations that shall arise in these electricities, from the reciprocal friction between the two internal surfaces of the ribbons.

950. By means of the precedent rules, I flatter myself that I have clearly expressed the laws that determine the nature of the electricities that result from frictions; now I shall endeavour to describe the manner after which these electricities take place. First, I shall propose two cases, simple and luminous, which shew, how immediately after a friction that has been performed in the air, electricities result in two ribbons, the one white, the other black. I shall afterwards avail myself of these examples in order to examine the electricity of other bodies: ribbons rubbed in the air, are electrified in a short space of time, and retain for a long while the electricity that is communicated to them.

951. I therefore hold with one of my hands, the extremity of a ribbon of fine white silk, three lines broad, and a foot, or a foot and an half long. I embrace both its surfaces with a piece of black velvet, very dry, or even warm, (warmth disposes bodies more plentifully to give their electric fire), and pressing the ribbon within the velvet, I make the latter move along the former; and after a short while the ribbon grows strongly electrified in a positive manner: another person mean while does the same with a black ribbon warmed, and a piece of white velvet, and the black ribbon becomes strongly negatively electrified.

952. Let us now proceed to examine the manner after which the electricity exerts or displays itself from rubbed ribbons, immediately after their friction: I think that it is the same as that in which it exerts itself from the plate of crystal formerly mentioned, (Pl. I. fig. 10.) I think that the *rubbing black velvet increases the capacity*

capacity of the white ribbon, and thus deposits in it such quantity of fire, as the friction may render it capable of receiving : but when afterwards the friction ceases on a given part of this white ribbon, I think that it recovers its less capacity, and that as the fire deposited by the black velvet then becomes *excessive*, it constitutes an absolute excess on the surfaces of the ribbon ; whence results its intense atmosphere. I likewise think, that in the act of the friction performed by the white velvet on the black ribbon, the latter diminishes the natural capacity of the former, and the black ribbon in consequence loses its fire ; but as that part of it which gets out of the friction, immediately recovers its natural greater capacity, there results a deficiency on both its surfaces ; and thence arises its intense negative *electricity*. Of this my idea I have, besides its analogy with other well ascertained facts, a proof from immediate experience. Another person and I hold a ribbon ; I rubb the one surface of it with any body whatever ; the other person presents a flaxen thread to the other surface ; and we find that it never moves towards the place that corresponds to the friction, it only runs, even obliquely, to the place where, on the other surface, the friction has ceased. This evinces, that in the ribbon as well as in the crystal plate, the electricity only begins to manifest itself, when the rubbed surface gets out of the friction.

953. And now, these considerations naturally lead us to propose the following questions. I. When I rub a ribbon on a plane, and when after the friction, the former adheres to the latter, does it in such state retain its electricity, or does it lose it, and only retain a disposition to recover it, when separated ? II. When the white and the black ribbon, by virtue of the contrary electricities which they have contracted, run to each other, or when either of them runs to join with the table, with the wall, &c. do they, in this state of adhesion, retain their actual electricities ; or can we say that they lose them, and only preserve a disposition respectively to recover them afterwards, in the act of their mutual separation ?

954. The opinion has, it seems, generally prevailed, that insulating bodies, when electrified and brought to the state of adhesion just mentioned, preserve their actual electricities ; and

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it was the more natural for every body to acquiesce to this opinion, as this kind of adhesion does only take place between two insulating bodies, or between an insulating body and a deferent one, when such insulating bodies, or body, are actually electrified, and as the continuation of this adhesion seems to evince a continuation of the electricities; besides, the nature of the electricity that again takes place after the separation, abstracted from other considerations, seems to prove the continuation of it, in the state of adhesion.

955. Yet, in the first place, I observe that in the instant when I separate, in the dark, a ribbon from the table on which I rubbed it, there appears a track of light on the successive places where the separation is made; a proof this, that the successive parts of the ribbon, as they are separating, begin to give signs of electricity, and to be in a different state from those parts which remain joined to the table, which give no sign at all. I therefore consider this track of light as a token, both that the ribbon had, through the friction, deposited its fire into the table, and that it recovers it, when it separates from it.

956. In the second place I also observe, that I have no sooner rubbed a white ribbon with black velvet, than it runs, if left at liberty, to my sleeves, to my coat, and sticks to them: nor is this all, but some parts of the said ribbon apply themselves and stick to such parts of the same, as already adhere to my body. Now, how is it possible to reconcile this union of the two parts of a ribbon, if both retained an homologous electricity, with the law, that bodies similarly electrified mutually repel each other.

957. In the third place, the phenomenon is still more instructive, of the reciprocal adhesion of a white ribbon with a black one, both contrarily electrified: this adhesion is inversely proportional to that electricity which remains in both ribbons, when thus united; this remaining electricity is of the same kind with that which, before the union of the two ribbons, predominated in either of them; and again the adhesion is inversely proportioned to this primitive excess. Let the white ribbon be more strongly electrified by excess,

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than the black one is by deficiency ; the ribbons in this case will indeed run to each other, but then mutually adhere with less force than if the deficiency in the black ribbon had been equal to the excess in the white one ; a redundant electricity will prevail in the two united ribbons ; and if the point of a needle be moved along the black ribbon, the two ribbons will after this cease to give any farther signs of electricity, the overplus of the electricity diffusing itself into this needle ; and the adhesion of the ribbons will moreover encrease.

958. Lastly, the fine experiment which Signor Cigna, a Physician, relates in the third chapter of his dissertation, entirely convinces me, that in the act of uniting an insulating body with a deferent one, or also an insulating body with one contrarywise electrified, both electricities are annihilated, and that when the two bodies are separated, the electricities are produced afresh. The said gentleman held a ribbon positively electrified by one of its extremities, and presented it to the smooth plain surface of a plate of lead insulated with silk threads, in a vertical plane : and he first observed that the ribbon was but slowly drawn by the plate ; which circumstance is conformable to my principle, that an electrified body does not join with a body that is not electrified, but after it has introduced into it an electricity contrary to its own : now, as the ribbon was positively electrified, it could not drive away the natural fire of the plate by means of its own atmosphere, but so far as the latter was imperfectly insulated, and in proportion to the intensity of the atmosphere which it was able to actuate on the opposite surface of the same. But if at the same time, that the above Gentleman presented the ribbon to the plate, he also presented his finger to the latter, a spark, he says, leaped from the lead to his finger ; the ribbon, at the same instant flew to the plate and remained adherent to it ; nor did either of them give after this any electric sign. Now, was not this spark correspondent and equal to the excess which the ribbon deposited into the lead ? Was it not therefore a sign that the ribbon, in uniting to the lead, gave up its own actual excess ? Lastly, Signor Cigna separated the ribbon from the lead, when the ribbon
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appeared electrified again; and then the same Gentleman, with his finger, returned a spark to the lead: that is, the ribbon, when taken from the plate, carried off part of the natural fire of it, leaving it in a deficient state, and thus recovered its own former excess.

959. Here we must observe that the above experiment is equally conclusive in any supposition whatever; because we must in all cases admit the two following propositions, viz. that two bodies similarly electrified repel each other, and that two bodies contrarily electrified attract each other. The spark which leaps from the lead to the finger, when the ribbon joins with the former, is able to electrify a third body in such a manner as to cause it to be repelled by this ribbon, when the latter is afterwards separated from the lead; this spark therefore is the electricity itself which belonged to the ribbon. On the other hand, the spark which leaps from the finger to the lead, when the ribbon has been separated from it, leaves such an electricity in that finger, as to cause it to be attracted by the ribbon, when it is thus separated from the lead; so that this electricity is a contrary electricity to that in the ribbon. Whence, in any supposed case, we may say that the ribbon in uniting with the lead diffuses its own electricity through it into the finger; in parting from the lead, it resumes this electricity; hence it happens that the finger afterwards finds the lead in a negative state.

960. From the above propositions and facts it manifestly follows. I. That electrified insulating bodies lose their actual electricity when they enter into a state of adhesion. II. That in the act of their separation, they recover it. Which principles being once laid down, cannot it also be said that this disposition which is left in the insulating body by its late electricity, of afterwards recovering it, is also the cause why this insulating body, while thus deprived of its electricity, remains in the above state of adhesion. But of this more accurate proofs will be given in the following chapter.

C H A P. II.

On the vindicating electricity of compact solid insulating strata.

961. **T**HE first phenomena that have been observed with regard to the *vindicating* electricity of compact insulating strata, were those, a notice of which was sent by the Father Jesuits at Pekin, to the Academy of St. Petersbourg, in the year 1755, and which may be read in the VIIth volume of the new commentaries of this Academy. Signor Symmer in his third Memorial, which was read in the Royal Society of London, the 20th of December 1759, says he charged two thin sheets of glass, joined together by their naked surfaces, and externally coated; when the charge was completed, he took the upper plate, by two of its angles, and when he raised it, he saw that the under plate stuck to it, and remained suspended to it; when he had discharged the plates, the adhesion ceased. He recharged the two plates, then having inverted them when thus united, he made the plate that communicated at first with the Chain, communicate now with the ground, and that which communicated with the ground, communicate with the Chain; when he found that after the electrization had, in this state of things, been continued a certain time, all adhesion ceased. Using afterwards two plates coated on both their contiguous surfaces, he found that no adhesion took place. Signor Symmer makes use of these two experiments in order to confute the theory advanced by certain philosophers, of two electric fluids, the one *affluent*, the other *effluent*; he pretends that each of the two distinct united glasses may be considered as the one of the surfaces of a single plate; that one of the glasses is impregnated with an electricity of one kind, and the other glass with an electricity of another kind; he moreover is of opinion that the adhesion of the two naked
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plates of glass is a demonstrating proof of the existence of two antagonist forces.

962. Signor Cigna, in the fourth chapter of his Dissertation, carried still farther the experiment of the Fathers of Pekin, and of Signor Symmer. He relates that two naked glasses, by rubbing the upper surface of them, remained united, both to each other, and to the gilt paper, or the sheet of lead, on which they were placed; that in this state they gave no sign of electricity; that if they were then separated from the paper, or the lead, they manifested on their two external surfaces the same kinds of electricity; that if the paper or lead was again joined to the glasses, the electric signs again ceased; that if the paper, or lead, was kept parted from the glasses by means of a silk ribbon, the paper or lead manifested an electricity contrary to that of the glasses; that if the glasses were likewise kept separated from each other, they also manifest contrary electricities.

963. I do not propose to repeat all the numerous experiments which I related in my book intitled, *Observationes atque experimenta quibus electricitas vindex latè constituitur & explicatur*. I am actually employed in promoting my enquiries on this subject, and if I meet with some success, I propose to publish what discoveries I shall be able to make. Mean while I shall only repeat in this place, the experiment which is made with the two plates, A B, *a b*, M N, *m n*, (Pl. XI. fig. 4.) jointly charged, and I shall express the successive effects of the *vindicating* electricity in this experiment with the figure 2.

964. And first, in order to perceive the unity which really takes place in all the phenomena of the *vindicating* electricity, however contrary to each other some of them may appear, it must be observed, I. That the law of the vindicating electricity of compact insulating strata, for instance, plates of crystal, is the same with the law of the *vindicating* electricity of rare insulating bodies, for instance, silk ribbons. II. That the whole specific difference between them lies in the former being capable of a charge, which the latter are not. III. Thence it results that the alterations of electricities, which are readily affected with bodies of a rare texture, by dis-
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joining and rejoining them, are not so with compact insulating strata; such alterations are confined to those surfaces of the latter which are kept joined together by the contrary electricities of the other two surfaces, which constantly endeavour to preserve *their contrariety to each other, and their equality with the electricity of the surfaces which are united together.*

965. For instance, I. Two ribbons contrarily electrified, when they unite together, reciprocally destroy their electricities, and thus remain adherent. After the same manner, if two plates AB ab , MN mn , are joined by their respective surfaces, $a b$, MN , contrarily electrified (I suppose the surface AB to be positively electrified, and the opposite ab , negatively; therefore MN is positively electrified, mn negatively) these two contrary electricities will endeavour to destroy each other; the redundant fire in MN will endeavour to diffuse itself into ab , and fill up its deficiency; but this reciprocal suppression of electricities cannot be effected otherwise than by a joint annihilation of the excess in AB , and of the deficiency in mn ; therefore, in consequence of the impenetrability of the plates, some external communication becomes necessary; and this will no sooner be procured, than the excess of MN will diffuse itself into ab , when the electricity of the two surfaces ab , MN , will be annihilated after the same manner as the electricities of the two ribbons were before.

966. Again, the two ribbons, when they are separating, freely recover their electricity, which they had readily lost when they joined; and in the same manner, the two plates MN mn , AB ab , in the instant they are separating, endeavour to recover on their surfaces ab , MN , the electricity they have lost in consequence of their union together, and of the communication of their external surfaces. Yet it is to be observed that the surface MN , in its endeavour to recover its excess, is restrained by the difficulty which the insulated opposed surface mn experiences in dismissing an adequate part of its own fire; and the surface ab likewise, in its endeavour to recover its deficiency, is restrained by the difficulty which the opposite surface AB experiences in recovering an adequate excess; whence it happens that the two disjoined plates,

plates, I. Manifest electricities reciprocally contrary. II. Similar electricities take place over the two opposite surfaces of the same plate. III. And this electricity is of the same kind as that recovered by the disjoined surface.

967. The reason is, that in disjoining the two surfaces *ab* *MN*, I. The surface *MN*, by endeavouring to recover its former excess, endeavours at the same time to drive away a quantity of natural fire from the opposed surface *mn*. Now, as the latter remains insulated, it cannot transfuse any fire into the ground, neither can it accumulate any within its coating *cd*; it therefore must accumulate it on the open surface of this coating, against the contiguous air: so that there will result an excessive tension in the natural fire of the ambient air, and a redundant atmosphere around *mn*. II. Likewise, in the act of the same separation, the surface *ab*, in endeavouring to resume its former deficiency, draws, according to the Franklinian theory, a certain quantity of redundant fire, to the opposite surface *AB*: now, as this surface remains insulated, it cannot derive this fire from the ground, neither can it draw it from the internal substance of its own coating; it must then draw it from the outer surface of this coating, that is, from the surface of the contiguous air (if before separating the plates, the coatings are taken off, the experiment will equally succeed.) Therefore, a particular relaxation will arise in the natural fire of the air around the plate *AB*; there will result a deficient atmosphere.

968. This explanation how the atmospheres arise, which take place over the surfaces opposite to those which are disjoining, likewise suffices to explain the singular circumstance of similar electricities arising over opposite surfaces of the same plates. If while the two plates *ABab*, *MNmn* are separating, two sharp points are kept presented to their external surfaces, the brush appears on the point directed to *AB*, and the star on the other which is directed to *mn*: the same force which, when the points are presenting, draws a brush to *AB*, and drives the fire that forms another brush from *mn*, this same cause I say, when these two surfaces remain insulated, draws to *AB* the natural fire of the contiguous air, creating a deficient atmosphere over it, and throws
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excessive fire from mn into the air contiguous to it, raising in it a redundant atmosphere.

969. That afterwards, over the external surfaces correspondent to ab , MN , when they are separating, atmospheres arise that are homologous to the electricity which these surfaces recover, is what appears natural, when we consider, *that the latter surfaces resume, by virtue of their separation, greater electricities than those which can possibly be raised on the opposite surfaces, which are insulated.* This principle being admitted, it follows that if the surface MN , cannot drive from the opposite surface mn , a quantity of fire sufficient to produce in it a deficiency equal to the excess recovered by the same MN , it follows, I say, that a portion of this excess must flow outward, against the contiguous air, and there produce a redundant atmosphere. Likewise, if the surface AB cannot draw to itself a quantity of fire sufficient to produce in it an excess equal to the deficiency recovered by ab , it follows that this AB must, from the air contiguous to it, draw a certain quantity of fire, and thus produce a deficient atmosphere over itself. That is to say, the excess redundant in, and flowing out of, MN , against the air contiguous to it, *ipso facto* lessens the excess in this MN , and thus brings it to a state of less unequality with respect to the deficiency actuated in mn ; and the fire which from the contiguous air flows into AB , *ipso facto* lessens the deficiency in it, and thus brings it to a state of less unequality with regard to the excess in ab .

970. These explanations of the *vindicating* electricities of two plates, may be demonstrated by the experiment in which, after jointly charging and discharging them, I continue for an hour and more to obtain sparks by touching them when separated, and again touching them when rejoined; and reciprocally, the above explanations throw a complete light on that same experiment, which I never could repeat without exciting the wonder of those who were unacquainted with electrical operations, and attracting the attention of the Philosophers who came to see my experiments. I join the two plates AB ab , MN mn together, by their naked
surfaces

surfaces in contact with each other ; and then introduce into the coating CD, for instance, the electricity of the Chain ; the charge being completed, I discharge them ; this done, I separate them, and touch the coatings ; I join them again, and then again touch them ; and thus doing, I continue to excite a very long series of sparks : here follows the manner after which I operate.

971. I begin with exciting sparks from the coating alone of the upper plate ; that is to say, I. I continually touch with one of my fingers the under coating *cd*. II. When I separate the plate AB, I take care not to touch its coating CD. III. Having separated this plate, I immediately touch it and give a spark to it ; that is to say, I give to AB an excess adequate to the deficiency contracted by *ab*, at the instant of the separation. IV. I cease touching AB ; I rejoin the two plates, and touch again CD, and draw sparks from it, by means of which I draw off the excess I communicated to AB after the last separation, and which it does no longer require, when in a state of conjunction. V. Proceeding thus with the usual caution not to touch the coatings in the act of separating, or of rejoining the plates, I continue to give sparks after every separation, and take them back after rejoining the plates.

972. In general, the spark which I draw after rejoining the plates, is more divided than that which I gave after separating them. In very favourable weather, after separating the plates, I often draw two or more successive sparks ; but after rejoining them, the fire that leaps from my finger is completely united into one spark, and much more vivid.

973. In order to understand the reason of this difference, we must consider, I. That the fire which flies from *ab*, in consequence of the deficiency which now takes place in it, goes to MN in order to form the excess which this MN wants ; therefore, as an excess arises in AB, in consequence of my touching it at times, so a deficiency arises in *mn*, in consequence of its constant communication with my hand. II. When I rejoin the two plates, the excess I have introduced into AB cannot be annihilated but so far as the excess in MN runs to fill the deficiency in *ab* ; and the excess in MN does not depart, but when I give fire to *mn*, in order to

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fill

fill its deficiency. III. In fact, if, while I rejoin the plates, I keep my finger at a distance from mn (or its coating cd) then I cannot draw from AB the excess I introduced into it; because as I do not then fill the deficiency in mn , the excess cannot be annihilated in MN , nor the deficiency in ab supplied. IV. However, when I touch mn (or cd) while I rejoin the plates, the excess of AB is not for all that thrown out at once, because the surfaces ab MN , do not instantaneously touch each other in all their parts; hence a slowness and successiveness take place in the respective annihilations of the excess in MN , of the deficiency in ab , and of the excess in AB . V. But when after separating the plates I present my finger to CD , (or AB) the excess is at once thrown to it from my finger, owing to the violence with which the whole AB then wants an excess adequate to the deficiency then completely formed in ab .

974. Conformably to what has been said above, we must take care that every time that the plates are joined, they be pressed together for some few seconds of time, in order that the small charges which have been formed by the separation, may have time both to dissipate entirely, and to arise again with more strength, when the separation will be again effected.

975. I. If after touching the plates when rejoined, they are again disjoined without drawing a spark, and then rejoined, no spark will be thrown from AB , because it has, in such case, received no fire. II. If after touching the plates when separated, they are rejoined, then disjoined again, without previously drawing a spark, AB then receives no spark, because it has given none at the time of its last joining with the other plate; *so true it is that insulating bodies contrarily electrified, are disposed, when they join together, mutually to annihilate their reciprocal electricities, as well as to recover them again, when they are separated.*

976. I have hitherto, in the experiment of the two plates, only examined that kind of electricity which is common both to compact insulating bodies, and to those of a rarer texture: I mean that kind of electricity, *by virtue of which they recover, when separated, the electricity which they had lost by their being joined together,*

together, and which I call *positive vindicating electricity*. Now, I shall in the same experiment, examine that kind of *vindicating electricity* which is proper to compact insulating bodies, and by virtue of which, when they are separating from one another, they give up the *electricity* with which they had been impregnated; this I call *negative vindicating electricity*.

977. Having therefore jointly charged the two plates A B *ab*, M N *mn*, I begin the operation of successively disjoining and rejoining them: in order to effect this more easily, I clip one of the angles of the one of the plates; and then I observe, I. That the plates, when they are disjoining, manifest signs of a *negative vindicating electricity*. II. They afterwards reach to the last limit of this *electricity*. III. Then successively follow, for a very long space of time, to give signs of a *positive vindicating electricity*. That is to say, I. At first, the surfaces *ab*, M N, when they are separating, lose a part of the *electricity* with which they are impregnated. II. Then they reach a certain term at which they do not, notwithstanding they are again separated, lose any more of the *electricity* which remains in them, nor recover any portion of that which they gave up when the *negative vindicating electricity* began to act, or even afterwards when the *positive vindicating electricity* began to take place.

978. In the meanwhile, the similarity of the atmospheres that take place over the two surfaces of the same plate, both when the *positive vindicating electricity*, and the negative one obtain, though it has been looked upon as fatal to the Franklinian theory, really proceeds from the following principle, which is the foundation of this theory, which is, *that the contrary electricities of plates, which by virtue of the separation of the latter, are become unequal on each opposite surface, severally endeavour to return to a state of equality; that is to say, that electricity on the one of the two surfaces, which the separation has caused to have grown less, endeavours to lessen the electricity on the other surface; and vice versa, that electricity which, in consequence of the separation, is become superior to its opposite one, tends to encrease the latter.*

976. Therefore, when I at first begin to separate the two plates $AB\ ab$, $MN\ mn$, the excess of MN and the deficiency of ab endeavour mutually to lessen each other; but the other two surfaces AB , mn , being insulated, their respective excess and deficiency are not altered; that is to say, the excessive fire is, as it were, drawn from MN into ab ; the deficiency in ab , thus becomes less than the excess in AB , and endeavours to lessen it; it therefore drives a portion of this excess in AB , against the air contiguous to it, and thus creates the redundant atmosphere over AB ; and reciprocally, the excess in AB being now greater than the deficiency in ab , endeavours to encrease it; it drives a part of the fire remaining in this ab , into the air contiguous to it, and raises over it a redundant atmosphere. Likewise, the excess in MN being become less than the deficiency in mn , endeavours to lessen it, it draws fire into mn from the air contiguous to it, and thus renders its atmosphere still more deficient; and reciprocally, the deficiency in mn , being greater than the excess in MN , endeavours to draw fire into the latter, from the air contiguous to it, and thus raises a deficient atmosphere over it.

980. On the other hand, when after the rise of the positive *vindicating* electricity, I again separate the plates, both the excess in MN , and the deficiency in ab , continue to be reproduced, though the contrary correspondent electricities cannot arise on the surfaces AB , mn , which remain insulated: therefore the greater deficiency in ab , endeavours to increase the lesser excess in AB , by drawing the natural fire from the contiguous air into it, and thus raises over AB a deficient atmosphere; and reciprocally, the less excess in AB endeavours to lessen the deficiency in ab ; to that end it draws fire into it from the air contiguous to it, and thus raises over it a deficient atmosphere. Likewise, the greater excess in MN endeavours to encrease the deficiency in mn , driving its fire from it into the air contiguous to it, whence results a redundant atmosphere over mn ; and reciprocally, the less deficiency in mn endeavours to lessen the
excess

excess in MN; to that end it drives a part of the latter's redundant fire into the air contiguous to it, and thus raises a redundant atmosphere over it.

981. Conformably to these principles. I. When I separate the plates $ABab$, $MNmn$, for the first time after their being charged, they resist so much the separation, that there is great danger of breaking them. II. From the coating CD a strong spark leaps to the nearest finger of that of my hands which holds the plate $ABab$, and the edge of its coating CD appears all round sparkling with very vivid brushes: all this demonstrates to me that a diminution of the excess of AB takes place, at the instant when the deficiency of ab is forcibly lessened. III. Likewise, in the act of the same separation, a strong spark flies from the finger with which I hold the plate $MNmn$, to its coating cd , and its edge appears all round shining with vivid sparks; this manifests to me that a diminution of the deficiency of mn , is effected at the same time that the excess of MN is forcibly lessened. IV. Meanwhile, the flashes of light which appear between the surfaces ab , MN , while they are separating, are produced by the fire which, by virtue both of the excess in AB which remains superior to the deficiency in ab , and of the deficiency in mn , which remains superior to the excess in MN , endeavours to leap from the above ab into MN . V. In this state of things, the upper plate $ABab$ repels a white ribbon from both its surfaces; over which, as has been explained in the preceding paragraph, similar redundant electricities take place. VI. On the contrary, the under plate, $MNmn$, repels a black ribbon from both its surfaces, by virtue of the deficient atmosphere, which as hath been also explained, takes place over both its surfaces.

982. The plates being joined again, the intensity of these attractions and repulsions lessen; because the excess of MN , and the deficiency of ab are now respectively kept back by the external deficiency of mn , and the external redundancy of AB . The adhesion of the plates takes place again, but in a less degree than formerly, proportionably to the diminution which the original charge has suffered from the first separation; and by

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ceeding to a second separation, the same phenomena continue to take place by virtue of the same causes as formerly, though their intensity is proportionably lessened.

983. Continuing thus to join and separate the plates, we pretty soon attain a term at which, I. The plates cease to manifest any sensible adhesion. II. In separating them no light appears. III. After the separation, they do not sensibly draw or attract rubbed ribbons. This term is the point of the contrary inflexion, the limit between the negative *vindicating* electricity which takes place at first, and the positive one which succeeds to it. This term is sooner attained, according as the insulation of the plates is less complete: in this case one plate sometimes reaches to this term a little before the other, which still continues to draw and repel ribbons with a sensible degree of force. Lastly, this term is attained, before the effect of the separations has entirely annihilated the charge introduced at first into the plates. In fact, if they are rejoined immediately after the term is passed, they still give pretty strong shocks.

984. If, after the term is passed, the plates are successively joined and separated, but without touching them; they begin, by virtue of these successive separations, to recover their former electricities; that is, the surface *ab* of the plate *AB ab*, begins to recover a part of what deficiency it had at first, and the surface *MN*, begins to recover also a part of what excess it may have lost. Whence it happens that, after the separation, the deficiency of *ab*, being become greater, endeavours to encrease the excess of *AB*, by drawing into it the natural fire of the air contiguous to it; and reciprocally, the excess of *AB*, being less than the deficiency of *ab* endeavours to lessen it, by drawing into the same *ab*, the natural fire of the air contiguous to it; so that *ab* and *AB* then begin to repel the black ribbon. Likewise the excess of *MN*, being become greater than the deficiency in *mn*, endeavours to increase it, by driving the fire of *mn* into the air contiguous to it; and reciprocally, the deficiency of *mn*, being less than the excess of *MN*, endeavours to diminish it, by driving the fire of *MN* into the contiguous air, whence *MN* and *mn* begin to repel
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the white ribbon. And thus the *negative vindicating* electricity becomes changed into a *positive vindicating electricity*.

985. By continuing thus to rejoin and disjoin the plates, those portions of electricity that had been lost are pretty quickly recovered on all sides, by virtue of these successive separations; the adhesion of the plates, and the repulsion of the ribbons also increase in proportion; so that it appears that all these phenomena of the *positive vindicating electricity*, continue till that degree is attained, at which the charges that had been introduced at first, are annihilated.

986. Beyond this term, if the plates are continued to be re-joined and disjoined, for an whole hour or more, without being touched, they continue to shew some adhesion to each other; they continue when separated, to repel ribbons conformably to the kind of electricity which they have resumed on their internal surfaces, &c.

987. I have represented in the fig. 2. of the Pl. XI. the series of the above alterations of the vindicating electricity. Now I shall make use of a figure of the same kind, in order to explain the vindicating electricity of the plate MN *mn*, (Pl. XI. fig. 4.) The same explanation will serve for the electricity of its fellow-plate; only, the ordinates must be taken on the other side of the absciss. Let the two equal right lines OF, *oF* represent the excess introduced into MN by the charge, and the deficiency introduced into *mn*. On the first separation of the plates, MN will, for instance, lose the portion *uF* of its excess: therefore, it will in consequence of this separation appear negatively electrified over both its surfaces; the plates being joined again, it will recover part of its former excess, and will thus be brought to have then the whole of its excess equal to P G. In consequence of a new separation, a portion *xG* of the same excess will again be lost; and thus it will at last happen, that MN will have that precise degree of excess at which a further separation can no longer lessen it; so that H is the point at which the *vindicating* electricity begins to be altered, that is, from negative becomes positive. At a following separation, by virtue of which the remaining excess is already reduced to the less value RI, the plate, instead of continuing

tinuing to lose any more of its excess, on the contrary begins to recover the portion of it Iy . Hence, as the remaining excess from the charge, in MN , is gradually reduced to the less values KS in K , LA in L , and o in M , the surface MN gradually recovers gradually greater portions of its former excess, Ks , Lz , $M\&$. From that point afterwards the surface MN , by virtue of other successive separations, will for a very long while continue to recover portions of its former excess, which (the operation being continued without touching the plates) will gradually vanish at every successive conjunction of the same.

988. And thus the portions of a curve OQM , oqM , will, with their respective ordinates, express the excesses and deficiencies, both primitive and remaining, of MN and mn ; the portions of a curve $uH\&v$, $VH\&V$, will, with their ordinates, express as far as H , the negative *vindicating* electricities, and beyond H , the *positive vindicating* electricities, of the surfaces MN , mn . The same portions of curve which serve to express the degrees of positive and negative *vindicating* electricities that take place at every successive separation of the plates, will also serve to represent the progression of the mutual adhesion of the plates. uF , UF will express the greatest degree of the adhesion of the plates, when they still retain their whole charge; which value will gradually lessen conformably to the successively lessening ordinates, xG , XG ; at the instant when the negative electricity will take place, this value will be o in H , that is, at the point of the contrary inflexion; and thence it will continue quickly encreasing, then very slowly decreasing, conformably to the successive ordinates. Iy , IY , Ks , KS , Lz , LZ , $M\&$, $M\&$, &c.

989. With respect to the experiments that are made on the *vindicating* electricity of a single plate $ABab$ (Pl. I. fig. 1.) by disjoining its coating CD , they differ much in point of intensity and duration, from the experiments that are made with the two plates jointly charged. Of this difference the cause partly at least is manifest: in the separation of the two plates jointly charged, the *vindicating* electricities of the two surfaces which are disjoining, co-operate together; and this circumstance must
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increase the effects, and better preserve the efficient causes; that is, the dispositions introduced by the charge of the plates, by virtue of which they endeavour to dismiss their respective electricities to a certain degree, and beyond this degree, to recover the same.

990. With regard to the manner after which the same vindicating electricities exert themselves, I observe, I. That positive *vindicating* electricities exert themselves after the same manner, when only one plate is used, and separated from its coating, as when both are used, and successively separated from each other. II. Negative vindicating electricities also exert themselves after the same manner, if the charge introduced into the single plate is very weak, consisting for instance, of only two or three sparks from the first conductor; because the charge which is usually introduced into the joined plates, is likewise small, on account of the thickness of the whole. III. But if the charge introduced in the single plate be much intense, then the phenomena which result from disjoining the coating of it, while the plate retains its whole charge, are proportionably different from the phenomena which result from separating the two plates, when they only possess their *joint* charge.

991. That is to say, each of the plates that retain their charge, manifests in consequence of a separation, the same electricity on both its surfaces, with that of the surface which is disjoined; but the plate which has been charged alone and possesses a considerable degree of charge, manifests that kind of electricity on the surface which is disjoined from its coatings, which is proper to that surface; and the contrary kind of electricity on the other surface. Thus, if the single plate *A B a b* be strongly charged, positively in *A B*, negatively in *a b*, it will, after the coating *C D* is taken off, repel a white ribbon from *A B*, and a black ribbon from *a b*.

992. The reason of this is, that charges universally endeavour, with a force proportioned to their intensity, to grow gradually less; and this force counteracts the force with which they endeavour

to keep their state of mutual equality, the force by which the single charged plate endeavours, when separated from its coating, to *actuate similar atmospheres in the air contiguous to its two surfaces*. When I take off the coating CD from AB *ab*, which I suppose to be strongly charged, I lessen the electricity of AB; therefore, by virtue of the force with which the two contrary electricities constantly endeavour to keep their state of equality, the deficiency in *ab* must lessen, and the excess in AB of course somewhat increase: as the electricity on both surfaces strongly endeavours at the same time to grow less in consequence of its very intensity, the deficiency in *ab* very strongly lessens by the united efficiency of the two above causes, and the excess of AB, even after the separation of its coating, will continue to decrease a little, in consequence of the lessening force, which arises from the intensity of its charge, and surpasses that which tends to an equality; thence, a certain quantity of fire flows from AB into the contiguous air; but *ab* at the same time draws fire from the air contiguous to it with very great force, and after this manner the above effects take place.

993. I have repeated the above observations from my above mentioned book on the *vindicating electricity*, and added some new ones, in order to throw some more light on the subject: with regard to the nature of the adhesion which accompanies *vindicating* electricities, I shall only subjoin two trials I have made about it. The first is as follows; if two plates, either charged, or lately discharged, and which therefore strongly adhere to each other, are immersed into an extensive subtle flame, or, when taken from this flame, are suspended within a large glass bell, emptied of air (Pl. X. fig. 11.) they soon part from each other. The other experiment is that of disjoining bodies naturally joined, for instance, strata or sheets of talc, or of *spato*: no electricity at all arises from these bare separations. With respect to the cause of the *vindicating* electricity, and of the adhesion that accompanies it, it certainly would, if discovered, throw a considerable light on the properties of insulating bodies, on the manner of their charges,

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Fig. 1.

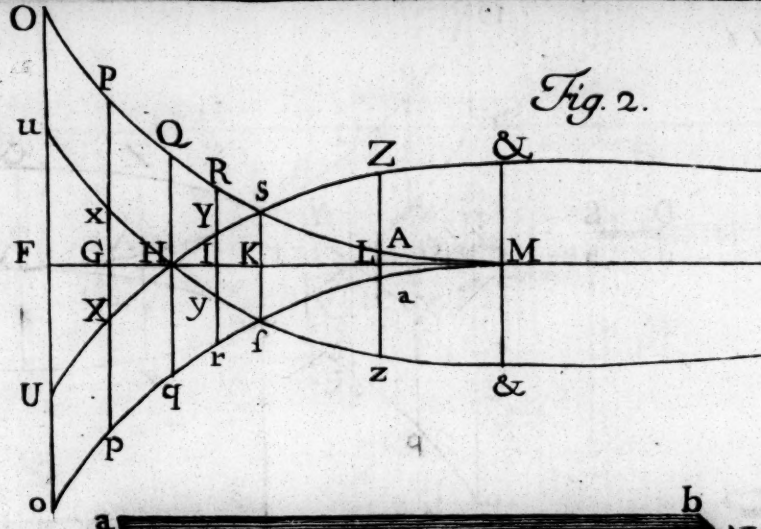
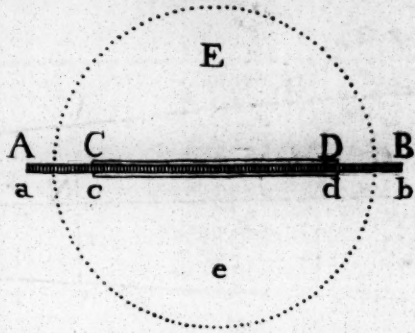


Fig.

3.

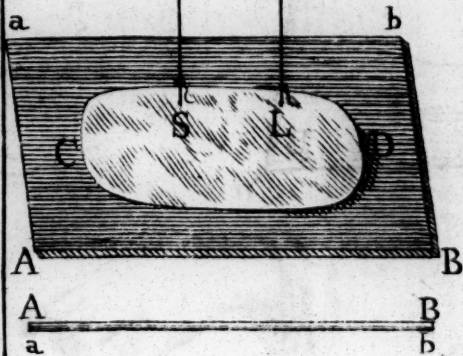


Fig.

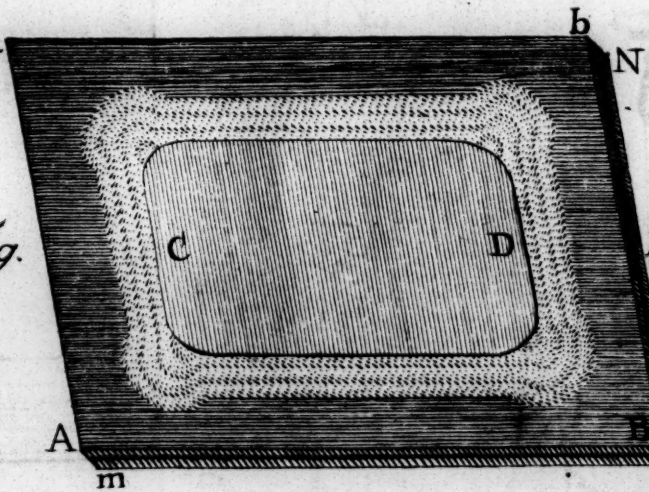


Fig. 8.

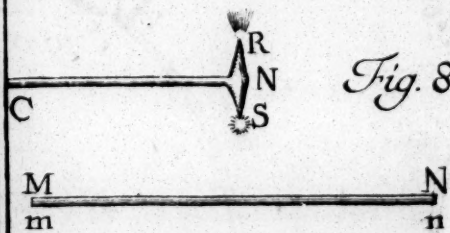


Fig. 5.

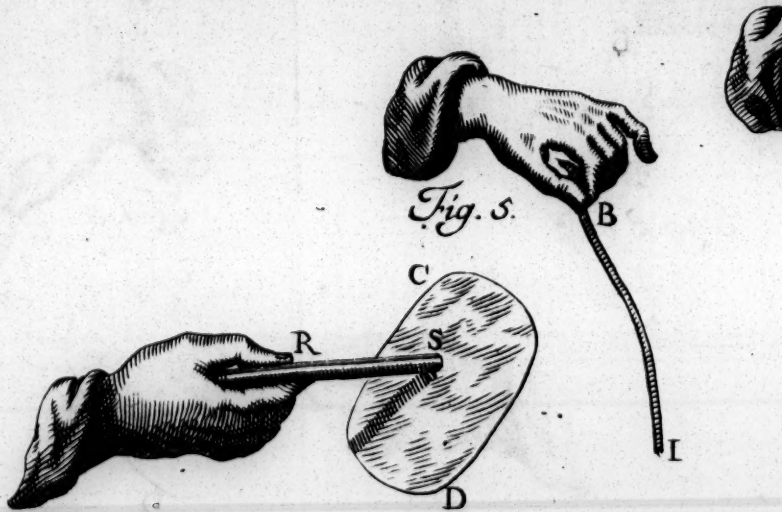


Fig. 4.

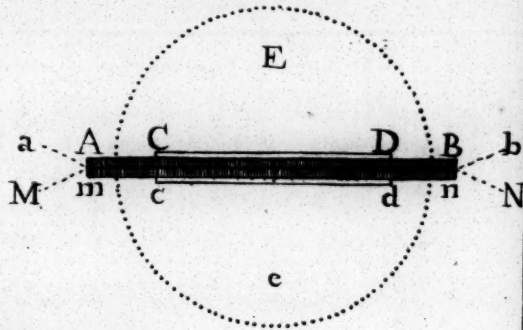


Fig. 7.

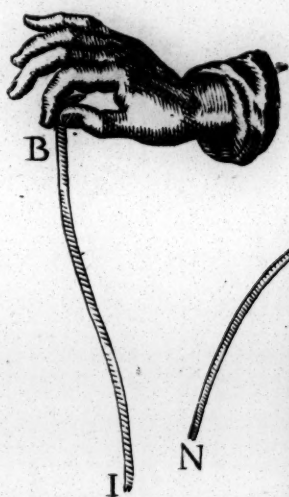


Fig. 9.

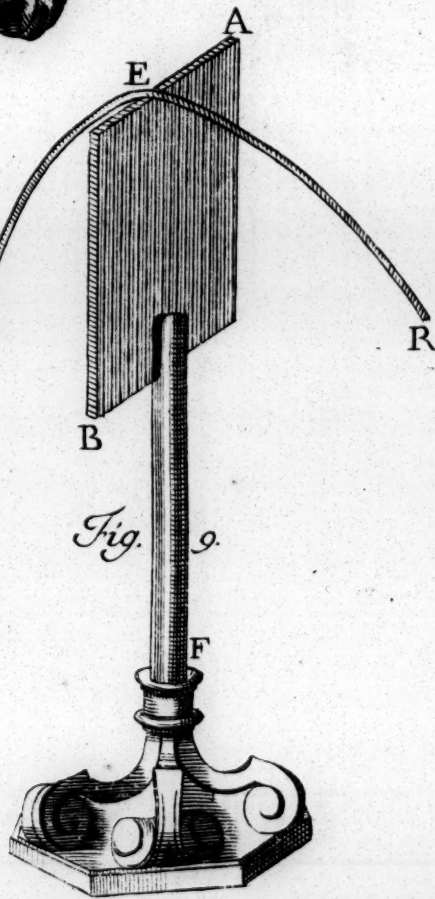
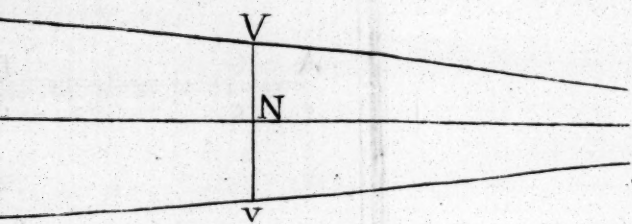
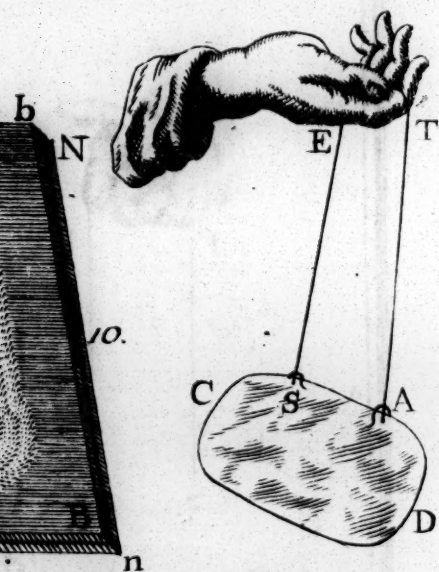
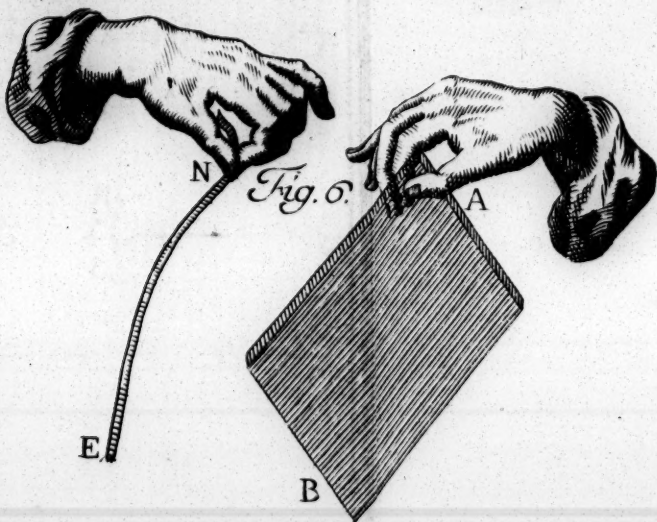
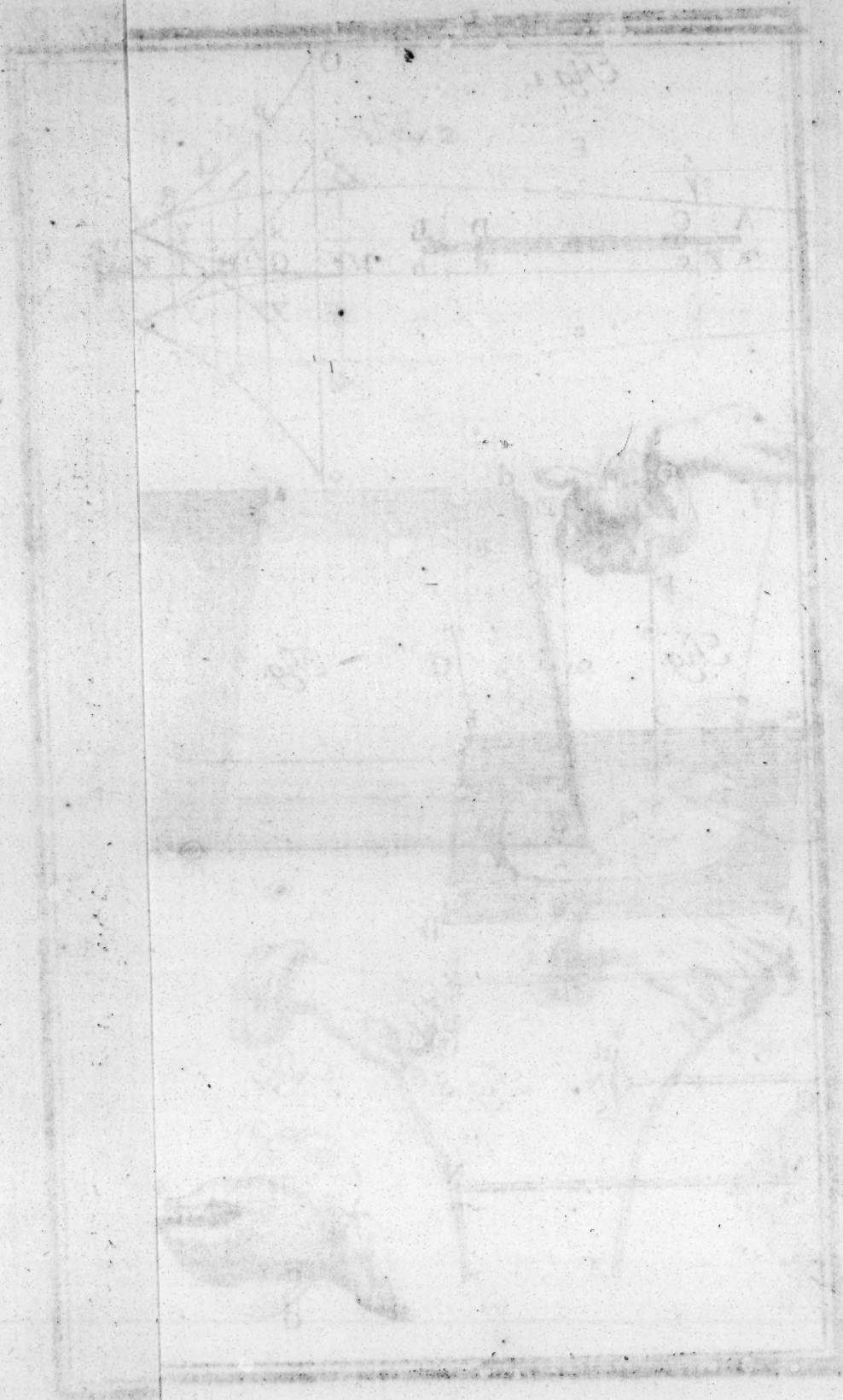


Fig. 6.





charges, on the nature of electric atmospheres, and consequently on all the most striking phenomena of electricity, such as the *brush*, the *star*, and the electrical motions. A consideration this, which is very apt both to excite us to investigate such cause, and restrain us from barely *imagining* it.

F I N I S.

A D V E R T I S E M E N T.

Though the following treatise on the state of Atmospheric Electricity, during serene weather, does not properly belong to the subject of the Work just now concluded, yet, as it is a late publication of the same Author, and its subject is both new and interesting, it has been thought it would not be disagreeable to the reader to find it added to this book. The numbers of the paragraphs have, in consequence, been continued from the preceding ones, as the Author himself has done, for the greater convenience of references and quotations.

O N

TERRESTRIAL ATMOSPHERIC ELECTRICITY,
DURING SERENE WEATHER.

Of its perpetuity and constant manner of acting, and of the alterations that take place in its intensity, in consequence of the changes in the state of the air.

L E T T E R I.

To his Exc. D. Angelo Maria Carrone di S. Tommaso, Marquis of Aigueblanche, First Secretary of State for Foreign Affairs, &c.

994. **A**S in the discharge of the high Office to which your hereditary loyalty, most excellent Sir, and your talents have raised you, you constantly endeavour to second the views of our gracious Sovereign, that is to say, to secure and promote the public happiness,—so your laudable taste for all that relates to natural history, as well as the deep questions you have of late proposed to me, besides the friendship with which you have constantly honoured me, induce me to hope that you will condescend to cast your penetrating eye on the following propositions, in which I treat of that perpetual motion which, in the atmosphere that surrounds us, obtains in that most active element, which is both the principal motor and performer of the greater part of natural meteores. If the propositions in question meet with such a favourable reception from you, which advantage I flatter myself they will obtain, their subject being both new and interesting, it will be a great encouragement to them to proceed farther, and make their appearance before the Royal Prince, to whom, amidst the universal and joyful congratulations of the people,

people*, it is likewise fit that Natural Philosophy should offer its applause.

995. First proposition. I. *How to settle a proper apparatus for exploring the electricity of the atmosphere, during serene weather.*

996. Ever since the year 1757, I have written at length, (and I think conformably to truth, as far as I can hitherto perceive) concerning the different properties of conductors, for indicating the state of the electricity of the atmosphere in general; which properties vary according to the substance, the length, bigness, shape, and height above ground, of these conductors. With respect however to the electricity of the atmosphere, during serene weather, it is to be investigated only by using *exploring* wires of metal, that are very long, and especially placed very high; for as such electricity is commonly very weak, it would, by operating otherwise, often prove a difficult matter to ascertain its existence, and much more so its nature.

997. For that same reason, viz. the weakness of the electricity to be examined, I do not recommend sharp points; they would too readily draw off and dissipate such electricity.

998. These previous essential cautions being laid down, I think I cannot better continue to indicate the manner in which a pretty good apparatus may be settled, than by shortly describing that with which I am observing this very instant.

999. In the first place, with regard both to the *openness* and height of its situation, it is settled on the pleasant hill of *Garzegna*, which is situated in the neighbourhood of Mondovi; and from which the whole compass of the Alps, as well as the whole plain of Piedmont, is easily discovered. This vast extent both of country and sky, that opens before the above hill, I take particular notice of, as contributing much to the success and certainty of the experiments.

1000. The substance of the apparatus consists of an iron wire, an hundred and thirty two French feet long, this I call the *exploring* wire. It extends from a stack of chimnies over which

* The Author means to speak of the circumstance of the marriage of the Prince of Piedmont, with a Princess of France, which was then on the point of being celebrated.

it is raised by a long pole, to the top of a cherry-tree to which it is fastened; its extremities are insulated, and over each a sharp small umbrella of tin is placed, the under part of which is coated with sealing wax. Lastly, another iron wire is fastened to the middle of the former; and being thence continued, and let into the room in which I observe, through a piece of crystal fixed in a wooden pane in the window, and secured in this piece of crystal with sealing wax, it brings me constant information of the state of the electricity in the *exploring* wire: I therefore call it, the *deferent* wire.

1001. This apparatus, which though it is much less extensive than those which I have hitherto used, either in this place, or in others, yet I find sufficient to discover the different motions of atmospheric electricity, during serene weather.

1002. Proposition II. *The electricity during serene weather, in its middle and ordinary state, makes two little balls of pith of elder, one line in diameter, diverge from a small sheet of metal placed between them, so far as six lines each: these balls are suspended by very fine silk threads, sixteen lines long.* II. *In the state of its greatest intensity, this electricity makes the above balls diverge so far as fifteen, twenty, or more degrees from the small sheet of metal.* III. *In its weakest state, it is only to be discovered by the mutual approach and junction of these balls, from very short distances.*

1003. I constantly measure the intensity of such electricity from the divergence it produces, and not from the sparks that are thrown off, because in its weakest state, it never rises so high as to afford any spark that are in any degree discernible; and even in its middle state, those it affords are only perceivable by a very dry sensible hand, or by presenting the tip of one's nose to the wire, in the meanwhile taking care not to breathe. When the electricity increases beyond this middling state, then indeed the sparks it throws are more sensible, but yet the difference between its successive degrees cannot be so well ascertained as by means of the simple divergence of the above balls; especially because an electrometer with sparks, would obstruct the continuance of the observations, as will appear from the following proposition.

1004. Proposition III. *The atmospheric electricity during serene weather is sometimes so slow, as that, being destroyed by the wires being touched, it takes one minute or more before it becomes again sensible; and at other times, it is so quick as to become again sensible after a second's time.*

1005. In expressing the above degrees, either of the intensity or frequency of the electricity in question, I mean to speak of such observations only as I have made with the above mentioned apparatus; for in proportion as the extensiveness, and height above ground, of the apparatus, will be increased, the intensity and frequency of the electricity, every other circumstance being equal, will also increase.

1006. Proposition IV. *The atmospheric electricity during serene weather, is always of the excessive, otherwise, positive kind. In the very rare instances that happen of this electricity being deficient or negative, it is then only brought over by the wind, from some part of the sky, though perhaps very distant from the place of the observation, which is either foggy, or snowy, or rainy, or cloudy.*

1007. Ever since I began to observe the atmospheric electricity during serene weather, the whole series of my observations has confirmed it to me, that this electricity is constantly of the excessive, or positive kind.

1008. On the mountain of St. Michael, I indeed happened three times in fifteen days, to find, during serene weather, the wires I had settled there in March 1767, (See Let. p. 134) electrified by deficiency. But then I took notice that the high mountains over the town of Susa, from which an impetuous squally wind was then blowing, were surrounded by clouds, the inside of which was much agitated, and their top was lengthened like rising smoke, towards the place in which I observed. The inhabitants of those mountains call those clouds *la gonfia* (the glass-blower) and they look upon them as sure signs that a certain wind, of a rainy, or snowy, and squally nature, which they call *la tormenta* (the storm) will take place: this wind, from those mountains blows eastwards. In Turin I never happened to observe an impetuous wind blow from the above mountains, but I also descried them to be surrounded by clouds

clouds like those I mention, lengthened like smoke, into right lines more or less extended; and reciprocally, I have always found such clouds to be accompanied by the kind of wind I mention.

1009. These observations I made on the mountain of St. Michel, hindered me from expressing myself in as positive a manner, in the letters I wrote to Signor Beccari, on the *nature*, as on the *constancy* of the atmospheric electricity during serene weather; because I was not sufficiently assured, that such an inversion of the nature of this electricity took place, only in the circumstance of the above rainy and stormy clouds.

1010. It has only been fifteen years after the above mentioned observation, that I have happened to meet with another instance of such inversion.—On the morning of the 19th of January 1770, having climbed up to the top of Superga, in order the more easily to observe from thence, whether the *Aurora borealis* of the night before had returned, I happened to meet there Sig. Prior Ceca, who, with his ecclesiastical studies, unites peculiar talents for sciences of other nature; and he, with great kindness offered to me to watch the motions of an apparatus which I had settled there. On the 24th of February, the apparatus was repaired; and I frequently afterwards took a trip there, in order to observe likewise, and see the result of the observations of the kind Prior. On the 13th of August 1771, to come to the point, being on the spot I mention, I happened to meet with another instance of *defective* atmospheric electricity, during serene weather: this circumstance we wrote, in the account we kept of our common observations, to have taken place, “during the time a very strong wind blew from the mountains, which towards North-west, hide the mountains of Lanzo, behind which we saw clouds rise, shaped like exhalations of smoke,” and in short, similar to those just now described.

1011. On a former occasion, Sig. Priore Ceca being alone, had observed another instance of a similar inversion of the state of atmospheric electricity; and it had taken place, (to use the very words of his journal) “at a time when a temporary cloud, brought
“by an easterly wind, broke, and was converted, as it appeared,

“into a *wind*, which passing over the wire, electrified it by a deficiency:”—by the word *wind*, which is a pretty common expression in this country, the Prior meant a cloud, the top of which stretched into several right lines, under the shape of exhalating smoke.

1012. No other instance of defective electricity, in clear weather, besides the above, I have succeeded to perceive, during three years I have since continued to observe on the mountain of Superga. Neither have I happened, in my observations in the Valentin, or in those which I have continued to make here in Garzegna, for several months every year, or in those I have occasionally made in Cigliero, Andrà, Alba, and other places, to meet with any instance of the like electricity, during clear weather. Only, on the 18th of April of the present year (1775), since I am again come back here to Garzegna, I have met with another instance of the same kind: here follows the manner in which I have set it down in my Journal. “About 10 o'clock 40' in the morning, the wind blows stronger than ever; the band-rolle or weather-cock, keeps waving with great quickness between north and east; I draw off a strong spark with my finger; in less than a minute I obtain another, and perceive that the balls are electrified by deficiency; they soon fall down, and then the wires again grow slowly and weakly electrified by excess; then again arises a slow and weak deficient electricity. At 11h. 5' a great divergence, certainly from deficient electricity, takes place; a little while after it lessens, but its nature is not altered. At 11. 10' the deficient electricity is very much increased, nor does its intensity seem to be regulated by the strength of the squalls of wind that take place. At 11h. 13, zéro; at 11h. 30', a small excess. . . . Barom. French inches 26, 5. 4. Hygrom. + 12. Therm. $12\frac{1}{2}$ (*Reaumur's Thermom. very likely*); wind north, and blows strong. In fact, the Apennine mountains are surrounded by clouds, the upper edges of which are divided into numbers of rectilinear spouts, like rising smoke, the tops of which are bent towards us; the clouds under these spouts look black, as in a storm; and before them, other clouds of an oblong shape are spread,

spread, which are flat underneath, and full of eminences on their upper part, &c ; at noon the wind continues to blow impetuously; small excess;" and so on.

1013. I have expatiated a little on the above few instances, in which, contrary to the general rule, the atmospheric electricity proved to be in a deficient state during clear weather, because I thought it very worth while to do so. Indeed every body knows how useful a thing it is in Natural Philosophy, to derive observations from Nature itself, and, by the assistance of a number of such, to be able sufficiently to ground some universal proposition. And how advantageous it is likewise, to lay down the true exceptions to the proposition in question, and to point out the peculiar circumstances in which they have taken place, so that the reason of such exceptions may thereby become sufficiently clear and manifest.

1014. And in fact, the different circumstances that accompanied the above instances of defective electricity during clear weather, sufficiently indicated how far they were exceptions to the general rule, that an excessive electricity constantly obtains in such weather, and shewed that such electricity was rather brought over, by means of the wind, from some part of the sky which was at that time either cloudy, or snowy, or rainy. In the instances I mention, the wind brought electrified vapours, in the same manner as Mr. Kinnerly sent electrical effluvia to his friend, by throwing his hat to him: only, the wind produced the above effect from an extremely great distance. In fact, during my last observation, the clouds I mentioned rose above the tops of the Apennine mountains, leaving their foot uncovered; and indeed their distance from the spot on which I stood was at least 64,000 *toises*.

1015. The above instance, which indeed is very rare, of electricity being thus brought over from a very great distance, does not after all, materially differ from those frequent instances in which, though the sky just over our heads may be clear, dark thick clouds, which draw continually nearer to the place of the observation, send an electricity which is found to be alternately

excessive and deficient. The edge of one of such clouds is, we may suppose, 20, or 30° distant from the place of the observation, and that part of the sky which lies over the latter, is, as hath just now been said, free from clouds; yet, under such circumstances, and at such a distance from the cloud, the exploring wire begins to be electrified (by excess and deficiency alternately) especially when the edge of such cloud is divided, and stretched towards the place of the observation, like exhalations of smoke. But instances like these latter, which frequently happen, I only mention in this place, in order that, being common and evident cases, they may serve to confirm the explanation I have given of the former, which occur but very seldom.

1016. Nor do I think however, that the six times I have happened to meet with an instance of defective electricity during clear weather, are to be considered as too small a number to draw any conclusion from. If it be considered that those instances, or exceptions to the constant electric state of the atmosphere in clear weather, are the only ones I have met with during a great number of years, and that every one of them has been accompanied by the same circumstance of distant dark clouds, resembling exhalations of smoke, it will be thought that those six exceptions, taken together, are a sufficient number for ascertaining both their *unity*, and the manner in which they were effected.

1017. Such inversions of, or exceptions to, the common state of atmospheric electricity in clear weather, grow still rarer, as the places are less raised above ground. Either at the Valentin during two whole summers, or at any time in Turin, I never happened to meet with such instances. Here in Garzegna, during the *vacations* of so many different years I have spent in it, I have only this year had an opportunity of observing the phenomenon in question; but then Garzegna is higher than Turin about one hundred *toises*. In Superga, which is 248 *toises* higher than Turin, I have twice observed the same fact. On the mountain of St. Michel, which is 583 *toises* high, I happened to make the observation three times in fifteen days. If the wind, before it reaches the place of the observation, strikes against the earth, it will

will bring no peculiar kind of electricity from the cloud from which it comes: now the mountain of St. Michel, which is like a high sharp cone, rises on the very entrance of the valley of Suza. Superga is not so high, but then it is situated at a pretty considerable distance a-head of the mountains in the neighbourhood, and it thus was the first which was met by those North-west, and East winds, from which I found the atmospheric electric deficiency proceed, which took place during clear weather.

9018. Proposition V. *To find the instruments that may serve to observe and ascertain the connexion between the atmospheric electricity in clear weather, and the present state of the air; and assign the precautions that ought to be used.*

1019. I find that the electricity during clear weather is constantly connected with the state of the air, as to moisture and dryness. Therefore, in the investigation I propose, observations of the barometer, thermometer, and anemometer, will be not only useful, but necessary: of all instruments however, that which is most essential is a perfect hygrometer, and such a one still continues to be wished for every day.

1020. That contrived by Monsr. Deluc came to my knowledge only a few weeks since. This hygrometer is composed of a very thin tube of ivory, to which a glass tube, like that of a thermometer is annexed: the mercury is made to rise in the latter tube, both by the heat, and by the dryness of the external air; which dryness contracts the tube or pipe of ivory. The mercury is likewise made to lower both by cold, and by moisture, as this moisture dilates the ivory pipe: the expedient therefore, imagined by the above Gentleman, has been to annex a corresponding thermometer to the hygrometer, and by its means, to ascertain and deduct that part of the motion of the mercury, which is the result either of heat or cold, merely: and from the remainder he measures the degree of the atmospheric moisture, or dryness.

1021. This hygrometer, among other advantages, seems indisputably to possess those of duration, and pretty exact mensuration: the author of it himself wishes it also possessed that of operating somewhat more quickly. This defect however may
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perhaps be remedied by some subsidiary hygrometer; and perhaps it will also be possible, with some other very delicate instrument, to find out the proportion between the real quantity of moisture, and the number of the degrees exhibited.

1022. However, being in absolute want of such a perfect hygrometer, I have hitherto made use of two, of my own contrivance: the one consisting of a string, this I call the principal hygrometer; the other of bits or shreds of straw, I call it the subsidiary hygrometer.

1023. The string is made up of thirty-two flaxen threads twisted together; the whole diameter of it is two third parts of a line; so that its thinness prevents its operating too slowly. I first try it by leaving it exposed to the open air for several weeks, with two pounds weight suspended to it: I then settle it on the outside of the window to which the deferent wire is brought, in the following manner. I fasten an end of it to a nail, and then make it go three times round a pulley which is placed twelve feet above the nail, and its other end is loaded with a weight. This string makes the axis of the pulley, together with an index annexed to it, move in company with the pulley; and the index shews the degrees of moisture, from $+ 20^{\circ}$ of moisture, to $- 20^{\circ}$ of dryness, on a round piece of paper, pasted in the inside of the room on a pane of glass in the window: every one of the above degrees is again divided into ten conspicuous parts.

1024. With regard to duration, an hygrometer like the above, commonly serves me for a year, or more; and I take care to make proper allowances, or corrections, when for instance I perceive, that, during very rainy weather, which continues with the same degree of heat, the hygrometer does not constantly indicate the same degree of moisture, &c.

1025. With regard to slowness of operation, I obviate it, as I said, with shreds of straw. With a sharp penknife I split the top of an old and well ripened stalk of rye into four parts; I take one of these, I strongly twist it, taking care not to break its fibres, and I insert it, thus twisted, through a square piece of wood, out of which it gets about two inches on each side: on each
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surface of the piece of wood degrees are marked, and over these very fine shreds of untwisted straw move, which are inserted through the body of the former twisted shred. This kind of hygrometer moves on first breathing upon it, and goes over several degrees, before the index of the other hygrometer with a string begins to move; it have found it particularly useful to discover the first moisture of the nightly dew; I for that purpose place it out of the window at night, in an horizontal situation, and the upper part of it indicates the degree of the moisture from above, and the under part indicates the moisture which any how takes place underneath.

1026. Yet, notwithstanding the care that may be used in that respect, hygrometers never can by themselves indicate the absolute quantity of moisture in the air, nor its complete relation to the state of the atmospheric electricity in clear weather.

1027. In the first place, every body knows how the air inclosed in a bottle of glass, drains the dew that has been introduced into it the night before; the whole atmosphere operates in the same manner; yet hygrometers may very well, owing to the heat of the day, indicate an increase of dryness, while the air is really taking in continually new quantities of moisture.

1028. In order therefore to conjecture with sufficient exactness the absolute quantity of moisture in the air, a thermometer will be useful, but much more so, an hygrometer which is sheltered from variations of heat and cold. However, by having a thermometer placed by your hygrometer, you may pretty exactly guess what quantity of moisture the external air requires, and can keep within itself in a state of exact dissolution, in consequence of its then degree of heat.

1029. I say pretty exactly, because there is another cause, besides heat, which makes air take moisture in, and keep it in a state of exact dissolution, that is, its density, owing to the weight of the atmosphere that presses upon it; and this additional moisture it deposits in a visible manner, when quickly dilated to a certain degree. Therefore, the variations of the barometer may likewise assist us in guessing with greater exactness that quantity of moisture, which

which the air actually requires and may keep to itself, in consequence of the above density.

1030. Besides the above kind of density of the air near the surface of the earth, there is another, which arises from a diminution of its heat. The same thermometer therefore, which by rising, indicates that the air begins to *require* a new quantity of moisture, may also by lowering, that is, by shewing that a condensation takes place in it, indicate the very same thing.

1031. To these considerations on the moisture of the air, another may be added, drawn from its insulating nature; which is, that it retains and stops the electric fire less exactly, in proportion as it is heated beyond a certain degree.

1032. In estimating this power of air, of *retaining* the electric fire, account must also be made of its different degrees of density. Thus, as the upper part of the atmosphere is, in fair weather, more completely free from moisture than the lower, it will in consequence thereof, more completely insulate than the latter; but then, owing to its greater rarity, it will also allow the electric fire to rush through intervals proportionably greater.

1033. I mention all these different circumstances, not that I think that observation can ever enable us to estimate them all exactly, and that any geometrical calculation of the motions of the electricity during clear weather, may be grounded upon them; but then it is not to be doubted but that paying a great attention to all the above different things, though without calculating them exactly, will prove of great service in investigating both the laws and the cause of the electricity in question.

1034. To the above extensive and nice considerations, great assiduity in observing must be added. Thus, to speak of myself on this occasion, whenever I have sufficient leisure to repair hither, to Garzegna, where besides other conveniencies, I meet with the advantage of solitude, I spend my whole time in observing. I live night and day in the high and open room I have chosen; there the *deferent* wire brings me incessant information of the state of the external electricity, and subjects it to a very sensible elec-

electrometer. From thence surveying all parts of the horizon around me, at every instant I have an opportunity of comparing the varying state of this electricity, with that of the weather or sky.

1035. By paying due regard to all the above mentioned considerations, and to several others I shall have occasion successively to mention, as well as by observing in the above mentioned manner, I flatter myself I have brought the complicated questions on the motions of atmospheric electricity during clear weather, to very simple terms, which I now proceed to lay before the reader.

1036. Proposition VI. *The moisture in the air is the constant conductor of the atmospheric electricity during clear weather; and the quantity of such electricity is proportioned to that quantity of the above moisture which surrounds the exploring wire; except such moisture also lessens the exactness of the insulation both of this wire and of the atmosphere.*

1037. I do not mean in this proposition to point out the principle itself which produces the electricity in question, but only to ascertain that medium in which it is inherent, and to the quantity of which it is generally proportioned.

1038. I think it will not be amiss to say something concerning the manner after which I have investigated the above truths: a few particulars of that kind will the more naturally lead the reader to the knowledge of the subject. And in the first place, I have endeavoured to procure, as nearly as I could, some kind of measure of the real quantity of electric fire, which in calm weather, manifested itself around the exploring wire.

1039. The following is a sketch of the electrometer I have employed here in Garzegna. I have before mentioned that I had fixed a piece of crystal in a wooden pane, placed in the window: this piece of crystal, which is pretty thick, has been bored through in the middle; and through the hole made in it a pretty long male screw is inserted, terminated outward in a ring, to which the hook of the deferent wire is fastened. To the other end of

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the male screw, which lies within the room, another screw is fastened, which terminates in a brass rod: this brass rod is of course placed in a perpendicular situation to the surface of the above piece of crystal; and upon it, by means of a proper support, the electrometer is raised. This electrometer, as I formerly said, consists of two small balls of pith of elder, one line in diameter; a small semicircular sheet of brass, placed perpendicular to the above brass rod, keeps the threads separated, that they may not entangle together; these threads are sixteen lines long, very fine and even, and I keep them wet with salted water.

1040. In order to measure the divergence of the balls, a semicircular piece of paper is pasted at some distance behind them, on the piece of crystal; this piece of paper is properly graduated, and parted into two quadrants, by an interval equal and corresponding to the thickness of the above sheet of brass.

1041. This kind of electrometer, which is extremely sensible, together with that of Mr. Elsmey, which is put in motion only by a stronger electricity, may enable an observer to measure the different degrees of electricity, with great exactness; especially, if the small pendulum of the electrometer of Mr. Elsmey be placed between two quadrants; for while it is placed over one only, part of the electricity of the latter is employed in repelling it, and yet it is from that part alone of the motion of the ball, which is parallel to the quadrant, that the whole electricity is commonly determined.

1042. In making the above observations on the electricity of the atmosphere during clear weather, I soon perceived that it was equally essential to make account of the frequency of this electricity, that is, of the velocity with which it arose again after being annihilated, as of its intensity itself. In fact, the quantity of the electric fire which accumulates in the exploring wire, continually varies, according to circumstances; and this quantity is not less constantly proportioned to the divergence produced by the electricity in question, than to the frequency of it. This frequency I have commonly computed from the number
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of seconds which elapsed before the balls again manifested any sign.

1043. In the first place, I have constantly observed that, whenever the electricity obtained, while the air was damp, whether such dampness arose from the heat of the air, which enabled it to take in a greater quantity of moisture, or its density, or whether it was accumulated only in the vicinity of the exploring wire, this electricity was more frequent, than when the air was dry to any great degree.

1044. In fact, while such a dry state of the atmosphere as above obtains, more than a minute is sometimes requisite to have the electrometer again manifest some sign of electricity; whereas in the damp state of the air I mention, (especially if such dampness proceeds from heat, for then it lessens much less the exactness of the insulation) a second of time has scarce elapsed when very rapid oscillations of the balls again take place between my finger, and the sheet of brass which stands between them.

1045. Such constant observation rendered it evident to me, that in the abovementioned damp state of the weather, a much greater quantity of electricity was exerted around the exploring wire, than in dry weather. But that same great quantity of electricity was but very inconsiderable, when compared to that quantity, which, must needs be dissipated from both the atmosphere and the wire, in consequence of their insulation being lessened by the dampness of the air we suppose.

1046. This conclusion which I drew by analogy from other electric experiments, was moreover immediately demonstrated to me by the different accidents I observed in the atmospheric electricity itself during clear weather. I constantly found that, whenever a dampness arose in the air from cold, especially when the vapours gathered and condensed near the surface of the earth, the frequency of the electricity indeed increased, but its intensity was lessened: now, this decrease of the intensity of the electricity, combined with the increase of its frequency, certainly indicates a very great deperdition of the same.

1047. Whence we are to conclude, that, however true it may be, that the quantity of the electric fire which is exerted in the atmosphere during clear weather, is proportioned to the quantity of moisture contained in it, yet, as this very moisture is the conductor of such fire, it thence follows, that whenever a somewhat considerable and lasting condensation of it takes place, every perceivable sign of electricity must cease in the exploring wire.

1048. Proposition VII. *The electricity that takes place, when the weather clears up, I. is always of the excessive kind. II. When the weather, in clearing up, moreover wants a great degree of dryness, and acquires it at a great rate, the electricity rises to a great degree of intensity, accompanied by a proportional frequency, which latter however grows less as the dryness of the weather increases farther. III. It sometimes happens that the electricity caused by the clearing of the weather, continues in its state of intensity for a long while, or also that, after being interrupted, it begins afresh; these accidents seem to be owing to such electricity being brought over by the wind from great distances.*

1049. As to the first part of the above proposition, it is confirmed by constant experience. I am so used to find the atmospheric electricity in an excessive state, when the weather is clearing up, that as soon as I perceive the thick lower clouds over my head begin to break, and the rare even clouds above them grow dilated, the rain having also ceased every where else, and at the same time find the balls of my electrometer diverge, I write down *excess, tendency to clear weather*; nor has it ever happened that the subsequent trials I made in that respect, shewed me that I had too hastily judged of the nature of the atmospheric electricity under such circumstances.

1050. The second part of the above proposition, concerning the peculiar intensity and frequency of the atmospheric electricity, while the weather is clearing up, is also conformable to a number of observations I have made in that respect: here follows the manner

manner in which, in my Letters, pag. 126 I described one of these observations, which I had made with a kite.—“While I was amusing myself with observing how, in proportion as the weather cleared, the briskness of the electricity encreased, I had the thought of running a pin through the pack-thread which held the kite, when I soon saw a very conspicuous brush of fire spring from its point towards my finger, the light of which continually encreased; so that, though I at last presented another pin to another part of the packthread, and a star appeared upon it, yet the former brush was not entirely suppressed.” This experiment lasted from one o’clock in the afternoon, till an hour after sun-set, and supplied me with a very favourable opportunity of observing the joint progress both of the weather in clearing up, and of the atmospheric electricity.

1051. The subsequent observations I have made with exploring wires, have since pointed out to me that circumstance which, when the weather is clearing up, particularly contributes to make the atmospheric electricity, which of itself is in an excessive state, to be moreover particularly intense; which circumstance is the same as that expressed in the present proposition, which is, when the weather, from a damp state, quickly passes to a state of considerable dryness.

1052. To the above observation, the answer is very conformable, which Prior Ceca sent to a question I had proposed to him, concerning the state of electricity, when the weather clears up. “If when the rain has ceased (the Prior said to me) a strong excessive electricity obtains, it is a sign that the weather will continue fair for several days; if the electricity is but small, it is a sign that such weather will not last so much as that whole day, and that it will soon be cloudy again, or even will again rain.” And in fact, it is very natural that a connection takes place between the state of atmospheric electricity, and the production of lasting fair weather, since the latter is effected by a complete dissolution of the atmospheric moisture; especially when such dissolution is quickly effected.

1053. And the observation I made just the day before yesterday, is quite conformable to the above prognostic. From the 30th of April till then, the weather had been rainy. "This morning, May 8, the weather begins to clear up; the hygrometer, (which very early was at $+19\frac{1}{2}$) is now, at 7 o'clock 30'm. at $+17$; detached clouds are passing over the observatory. Small excess at eight o'clock, 40m. the return of fair weather is decided; the clouds are gradually dissipating, though no wind blows, &c. In consequence thereof, I find that the electricity is so increased that the balls of the electrometer diverge 20° ; in its highest state it reaches to 25° ; but it frequently falls quickly, nor does it rise again, but after a minute of time. . . . The sky is without clouds to the distance of about 45° degrees from the zenith. I think that the above quick falls of the balls arise from the moisture in the air, which during the clearing of the weather, is depositing on the glass-sticks by which the exploring wire is insulated, which moisture produces a kind of imperfect communication on the surface of those sticks, which only allows the above electricity to dissipate itself through it, when it has risen to the above degree of intensity. A great calm still continues to prevail, so far as I can judge from the bandrole of the weather-cock. At 9 o'clock 50m. the electricity rises again to 25° , then begins to fall. . . then rises again. . . . These alterations continue till 11. 15'; afterwards the intensity of the electricity lessens, as well as its frequency; the hygrometer stands at -6 At noon it stands at -10 . . . In the afternoon the divergence is of 12° ; the hygrom. stands at $-12, 2'$; about sun-set the divergence is of 8° ."

1054. The above alternative falls of the electricity serve to show the cause of that particular kind of electricity which obtains when the weather is clearing up, which is the quickness with which the moisture in the air is then dissolving. To this the reviving of such electricity, which rose to 20° , or even to 25° , only after a minute's time, is very conformable: when an electricity obtains in dry weather, it is a very common thing that a longer time than that elapses, after it has been destroyed, before it again rises

rises only to 6° . However, I will add with respect to the above prognostic, that the weather continued fair yesterday, and to-day.

1055. Lastly, the third part of this proposition has for its object, a kind of electricity analogous to the former, which, after the weather has completely cleared up, continues to obtain, or sometimes will rise afresh: such accidents I have pretty constantly observed here in Garzegna, when a cold wind blew, caused by the snow that had fallen on the neighbouring mountains. Thus, on the 4th of October 1769, the electricity of *clearing weather*, which here had ended about noon, began afresh about three o'clock, and I wrote, as I find in my Journal, that, "probably the electricity of clearing weather, on the mountains, which continued clouded pretty late in the day, did only reach us in the afternoon; and that the above cold wind was blowing from these mountains."

1056. In the last proposition I have considered the state of electricity during the clearing of the weather, which is the one of the limits between cloudy and serene weather. I propose now to consider its state, while clouds are gathering, which is the other limit between serene and cloudy weather.

1057. Proposition VIII. *If, while the sky grows clouded over the place of the observation, only a high cloud is formed, without any secondary clouds under it, and such cloud is not an extension of a cloud that drops rain elsewhere, in such case I say, either no electricity takes place, or it is an electricity of an excessive kind. If the clouds which are gathering, are shaped like locks of wool, and continue in a state of motion from and to each other, or if the general cloud which is forming lies very high, and is stretched downwards like descending smoke, then a frequent excessive electricity commonly takes place, which is more or less strong, in proportion to the quickness with which this cloud is forming, and it foreshows the greater or less quantity and velocity of the rain or snow which is to follow. II. When a rare, even, and extensive cloud is forming, which darkens much the usual colour of the sky, and turns it into a grey colour, an excess of peculiar both intensity and frequency takes place; but in proportion as the gathering of such cloud slackens, this excess lessens, or even fails.*
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On the contrary, if the rare extensive cloud we suppose, continues to be gradually forming of smaller clouds, likelocks of wool, which are continually joining to and parting from each other, the excess commonly continues.

III. *Low and thick fogs, (especially when, in their rising, they find the air above them pretty free from moisture) carry up to the exploring wire, when they reach it, an electricity which becomes manifested by frequent little sparks, and produces a divergence of 20° , 25° , or even 30° . If the fog grows sluggish, and continues around the exploring wire, all electricity soon fails: if it continues to rise, and another cloud succeeds, it brings to the wire a fresh electricity, though less than the former. Sky-rockets sent through such thick, low and continued fogs have often afforded me signs of electricity. However, I never happened, under any one of the circumstances above described, to meet with an instance of defective electricity; except perhaps once, when I sent a sky-rocket (to which, like to those above, a long string was fixed) through a low thick fog; though I had afterwards every reason to think that I had mistaken a false little star, for a true one.*

1058. With respect to the first part of the above proposition, viz. the constant prevalence of an excessive electricity, in the circumstances above described, I have ascertained it but lately. The several manners in which clouds are formed, are complicated with numbers of different accidents, which I have been obliged closely to observe for many years; and it has only been by using much patience, that I have been able to discern essential and characteristic differences between them, and thus to derive some real information from my numerous observations.

1060. In regard to the second part of the above proposition, viz. the formation of those thin, even, extensive clouds, which for the time being, make the sky appear of a grey colour, I have made abundance of observations, with the apparatus which I settled on the Valentin, in the spring of the year 1757. On the 11th. of May the very long rope was fixed, which extended over the river Po, from the Villa of the Mission, to the Valentin; and from this rope, an iron wire was continued to the portico of the botanic garden: here follow the words of my Journal, as they were written by Sig. Canonica, who assisted me in my observation.

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"On the 12th of May, at 2 in the afternoon, frequent small sparks take place, while the sky is overspread with whitish vapours." Again, "on the 23d of the same month, a perfect calm; the sky is overspread with whitish vapours; the electric signs prove either strong, or weak, according as the sky is more or less copiously covered with such vapours."

1061. In my first observations on Superga, I took notice, in the same manner, of the particular kind of electricity which is described in the second part of the present proposition. The apparatus had been fixed on the 24th of February, 1770. "On the 28th the sky having been, at sun-rise, pretty clear and free from clouds, the atmosphere begins to be loaded with a rare fog, which condensates as it rises; while it thus keeps rising, and between 6 and 7 in the evening, the wire affords frequent signs; afterwards they cease, the above said rare cloud being already very high, and converted into numbers of small whitish clouds, united to each other. On the first of March, at 9 in the morning, the air begins to be loaded with vapours, yet they are so rarefied as to form no cloud; the wire is electrified by excess; these vapours grow gradually thick enough to make the light of the sun appear of a whitish colour, and both the briskness and frequency of the signs increase; about 10. 45'. the vapours continue to rise, and several whitish streaks, formed by little clouds, begin to be formed, when the electricity so increases as to afford six or eight small shocks, every minute. On the second of the same month, in the morning, the vapours still continuing to rise, the electricity proves the same as yesterday, &c. On the third, about 10, the wind falls; at 10½ the wire resumes a middling degree of electricity, which lasts in that state the whole day; no well formed clouds, but only a great quantity of vapours appearing in the air," &c. The above observations have since acquired a continually new degree of precision, from those I have made in the Valentin, on Superga, here in Garzegna, and in the Astronomical Observatory in Turin.

1062. I shall dwell somewhat longer on the third part of the present proposition, viz. the electricity produced by a thick fog

which rises ; as it has been the subject of my first experiments, which began in the year 1756, and which I have described, in my first Letters to Sig. Beccari. (See *Letters* to Sig. Beccari, num. 67.)

1063. "On the second of December, after a low thick fog had prevailed for several days, which had kept both sun and stars constantly hidden, and the weather continuing in that same state, so that a man could not be perceived at the distance of thirty steps, I raised a sky-rocket, about $2\frac{1}{2}$ in the afternoon ; the first made the flaxen thread move immediately, and with great force ; indeed, this thread, which was of itself much wrinkled, became stretched into a completely right line, and so did all its different filaments : I moreover could perceive a light on its lower extremity."

This light suggested the idea to me of using the electric lanthorn : I warmed its neck, and carried it to the place of the experiment, wrapped in warm cloths, which were to be taken off only after the sky-rocket was raised. Having sent up the rocket, I easily perceived a real *little star* within the lanthorn, which corresponded to the string which was fastened to the rocket : this little star, however, I only perceived after the rocket had risen to a great height ; nor did I see it but during an instant."

"On the third of December, the weather continuing in the same foggy state, except that the fog was somewhat higher, I again sent up two rockets, and the same accidents took place."

"On the fourth I repeated the experiment, in the Hall of experiments in the University, in order to exhibit it before the Students of Natural Philosophy, who come there at settled days ; when the experiment succeeded very well ; only the electric signs were neither so quick, nor so strong as in the former ones : the fog was in great measure vanished ; the sky indeed continued to be overspread with clouds, but they lay high, and began to dilate : the above experiments was made between 9 and 11 in the morning ; and at noon, after twelve days of foggy and cloudy weather, the sun made its appearance."

1064. These experiments on fogs, which were the only subject of the Letters I wrote to Sig. Beccari, are but few, when compared to those I continued to make, during the winter of that same year. "In order (I said in one of these Letters) to avoid the tedious narrative of all the numerous attempts and experiments I have, till now, continued to make, I shall only indicate here the different methods I have used for making such experiments, leaving for another occasion the account of their precise results, (*Let. num. 73.*) and in the first place, I have during the course of the last winter, frequently raised rockets, and explored the electricity of the atmosphere, conformably to the method described in my last." Now, that phenomenon which most excited my curiosity was the electricity of fogs.

1065. During that same winter I had made experiments upon fogs, only with rockets. The first time I have observed their electricity with wires, was towards the end of that winter (*Let. num. 75.*) "On the latter end of March, I climbed the high and steep mountain of St. Michael, and there I stretched and insulated several iron wires; the one in the direction of the Meridian of the Monastery, to the ruins of the *Sepulchre*, which was 1600 feet long; another 120 feet long from the same place, and in the same direction, to a battlement of the Monastery; another, 112 feet long, at the same height, and eastwards, to a store-house; another 160 feet long, from the steeple to the ruins of a portico, westwards; and lastly, I fixed a pole on the top of the Church, on which I had insulated a metallic point with a glass stick, and another iron wire 40 feet long, extended from this point to the neighbouring steeple."

1066. It was then I say, I had, for the first time, an opportunity of observing the electricity of fogs, with exploring wires: I had settled them on such a high spot, and made them of such great extent, in order constantly to obtain, by their means, the same electricity as I had till then sought to procure with kites, which can only be used when the wind blows steadily. One day, to come to the point, as we were preparing to leave the mountain, and were going to take our wires down, a fog arose

close to the east side of the mountain. Having perceived this, Sig. Borghefi, who is now a Physician at Saluzzo, and I, ran to the wire of the store-house, which the fog had scarcely begun to surround, when it afforded little sparks, continued and pretty brisk; but they soon ceased, having lasted rather less than a minute: they began afresh twice, but continued a less time, and were weaker than the former; which was owing to the fog then leaving the wire: and as that which succeeded it, grew sluggish and condensed itself around both the wire and the whole mountain, it put an end to all electricity.

1067. This observation created in me a greater desire of renewing it, than of giving, as yet, any public description of it. In the Valentin, either during that Summer, or the following, I had no opportunity of meeting with a like instance, especially because I could not go there early enough. As for this place, Garzegna, it is but a bad situation for observing fogs; they either never reach it, or only do so in cloudy or rainy weather.

1068. With respect to the other observations I have attempted to make, here in Turin in the Astronomical Observatory, when it was first established, and in Superga, I shall describe them on some other occasion: I mean here to speak of fogs, no otherwise than as one of the limits between serene and cloudy weather: however, since I am treating this subject, and two ingenious English Gentlemen, Mess. Ronayne and Henley* have also of late investigated it by different methods, I shall here subjoin a few questions I proposed about it, in my Letters to Sig. Becari, num. 451. "Now, do not low, sluggish, and thick fogs also depend, at least in part, on the aerial electricity? It is enough for me to consider that neither the ground, nor the walls, nor cieling of my room, prevent my giving such electricity to the air in it, as lasts for a very long while, to understand how, notwithstanding the contact of the earth, an aerial electricity may exist, extremely apt to raise and actuate the above

* The above gentleman is the same, who, owing to a mistake in the Italian original, has been called *Elfmey*, in the 104th paragraph, p. 434.

mentioned fogs."—I then continued, num. 452, "Certainly, among the many effects which aerial electricity may produce, the following is abundantly confirmed by experience, viz. that all the vapours, or moist effluvia whatever, which are any how brought to rise in the atmosphere, or which swim, or descend in it, are affected by the aerial electricity, in their absolute as well as relative motions. Thus for instance, several admirable accidents of the dew, or of the hoar-frost, the tendency of their drops, or icicles, to certain particular bodies,—this tendency obtaining from all sides, though more particularly directed to the angles, edges, and points of such bodies, all these are accidents which suppose a perpetual electricity in the atmosphere, &c.

1069. Proposition IX. *When, during clear weather, a cloud happens to pass over the wire, which is low, tardy in its progress, and single, that is, considerably distant from any other, the electricity by excess commonly weakens much, but does not turn into a defective one; and when the cloud is gone, it returns to its former state.* II. *When numbers of whitish clouds, like locks of wool, keep over the wire, continually uniting with, and parting from, each other, and thus forming together a body of pretty considerable extent, the electricity by excess commonly increases.* III. *In all the above circumstances the electricity by excess never turns into a defective one.*

1070. In the two preceding propositions I have treated of the two limits between clear and cloudy weather, viz. its clearing up after rainy weather, and its growing cloudy after fair weather: in this, I consider the two different manners in which this latter change is effected. The whole of my successive observations authorises me to lay down an essential distinction in that respect, and to indicate what clouds increase, what lessen the electricity; a thing about which I expressed myself but incompletely in my Letters to Sig. Beccari, (pag. 167.).

1070. The clouds which weaken the electricity are those which proceed slowly: for I have in different instances happened to see, during clear weather, portions of clouds which the wind had detached from the body of those around the mountains, and which it transported rapidly and pretty exactly united into one body,

body, over the exploring wire, remarkably increase its electricity: the same I find, has also been observed by Sig. Prior Ceca.

1071. Such clouds, moreover are low; for all those which I have found more remarkably to lessen the electricity, appeared to be much so, from the great distinctiveness of their parts, the darkness of their lower surface, and the limits to which this darkness was confined, relatively to the then situation of the sun.

1072. Lastly, I farther distinguish those clouds by saying that they form only one body, and are entirely severed from other clouds. With respect to all those characteristics of the clouds I mention, they are very easily distinguished. Number of such clouds, for instance, are to be seen in summer-days, especially when showers of rain take place at no great distances; they are not much raised above the horizon, and are disposed seemingly in pretty regular order, at a sufficient distance from one another; their upper parts are covered with prominences, and their under surfaces are kept flat by the wind which conveys them: I commonly express them by the short appellation of *rafts*, which fits them the better as they bring supplies for the above showers. Such are the clouds, I say, which passing over the exploring wires, weaken their electricity, after which it returns to its former state, as I have frequently observed in the Valentin, and here in Garzegna; nor have I failed to perceive the same effects in Superga, whenever I have happened to be there, in proper weather.

1073. I have not less frequently, nor with less pleasure, stood contemplating those clouds of another kind, which are of a whitish colour, and are in a continual perturbed motion: for which reason, as well as on account of their peculiar appearance, our countrymen call them the *kids*. When one considers them attentively, he soon perceives that they are not kept in that state of intestine motion by the action of the wind, but by some peculiar internal principle which causes them to join together by some one part of them, and to part in another. Now, such clouds, contrary to the former, commonly increase the electricity of serene weather. However, as their first rise, as well as final dissipation, are never so conspicuous accidents, as those of the
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passage of the above clouds, or rafts, so neither can the beginning, as well as end of the change they produce in the present state of the atmospheric electricity, be so well ascertained as that brought on by the former.

1074. Such are, most excellent Sir, the phenomena exhibited by the atmospheric electricity during serene weather, the perpetuity of its existence, the constancy of its nature, its connexion with the dryness and moisture of the atmosphere, its prevalence even under the circumstances of the weather beginning to grow cloudy, or clearing up, and the different opposite changes that are produced in its state, by solitary clouds of different kinds. Though the above different observations and facts may at first sight appear in great measure remote from the important questions your Excellence has proposed to me, yet, I flatter myself that I may be able, in time, to shew the close connexion there is between them, and will thus be so happy as to have once more an opportunity of expressing my great respect and gratitude to your Excellence.

Garzegna di Mondovi,

May 16, 1775.

L E T-

L E T T E R II.

On the Daily Period of the Atmospheric Electricity during Serene Weather.

To the illustrious President of the Royal Society of London,
Sir John Pringle, Bart.

1075. **I** do not know how better to express my gratitude to several Members of this Royal Society, for the kindness they have at different times shewn to me, than by expressing these sentiments to the Society itself, by your means, illustrious Sir, and making an offering of some specimen of the researches I have made concerning the most active of elements. My inclination for the Science of Electricity, which Science properly belongs to this century, has never suffered any diminution; and I should be particularly happy, if besides the satisfaction of pursuing a favourite study, I have had the advantage of deserving the approbation with which the lovers of electricity in general, and the founder of it, the immortal Dr. Franklin, as well as the most ingenious Dr. Priestley, who so successfully promotes the knowledge of both electricity and the element we breathe, have honoured my former endeavours. Indeed, I hope that the new discoveries I now offer to the Public concerning the nature, the constancy, and other different phænomena of the atmospheric electricity in clear weather, will not be found by the above persons, by any means unworthy of the time I have employed in my long and numerous researches. The mild electricity I mention is not in itself less to be regarded, than the stormy and thundering: and the slow, but continuous effects of the former, are of no less importance than the violent and loud operations of the latter. Every thing in Nature is great; nor is it any diminution of the merit of a discovery, that the objects discovered do not make upon our senses any strong impressions.

pressions. With respect to the mild and constant natural electricity, which is the subject of this treatise, I moreover flatter myself, that an attentive investigation of its effects will be in the highest degree useful to attain a knowledge of several important circumstances relative to the other kind of natural electricity, which are unknown to this day.

1076. In the first place, in order to imitate the method of the ingenious persons above named, I will shortly relate the manner in which I have been led to make the observations which I propose to offer to the reader, and in which I have afterwards promoted them. So early as the year 1753, in my first book on Electricity p. 173, num. 544, and following, I offered conjectures, "that several phenomena which daily take place in our atmosphere, may be explained by means of an electricity of a weaker kind." But this reasoning was no more than a conjecture, which I drew from several accidents I observed to take place in the dew, and from the constant manner of Nature, which leaves no agent unemployed, and carries operations of every one to every possible degree. I then observed only with the common apparatus, which as it was moreover in a very indifferent situation, only could indicate to me the higher degrees of intensity of the atmospheric electricity during serene weather; besides, that my observations on that electricity which takes place during showers, and rain in general, kept me then constantly employed.

1077. It was in the year 1756, that the frequent and continued use of kites, which other observers only used to make researches on the electricity of clouds, procured me a confirmation of what I had till then only conjectured, that is to say, that even during clear weather (except in the cases of a great dampness of the air, or of an impetuous wind) a mild weak electricity perpetually took place. (Let. p. 166).

1078. Kites were most useful instruments to me, for such first experiments on the state of the atmosphere. They rise to a great height, to a region where the difference of the atmospheric

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electricity uses to be greater; they gather great quantities of this electricity, by means of the long pack-thread which holds them, and they retain it the better as they are capable of being exactly insulated. With respect to insulation, experience has suggested the following formule to me, $E = \frac{D, R, L.}{S.}$, that is to say, the exactness of the insulation is proportioned, I. to the dryness of the air, directly; II. To that repulsion of dampness, which is proper to insulating bodies, directly. III. To the length of such bodies, directly; and IV. To their section S, inversely, or to their perimeter, if they are solid compact bodies. Now, a string made of the best silk, of a small diameter, and of great length insulates a kite extremely well; and it is an easy matter to keep it dry by warming it, or to change it, when it grows damp. Though I was at first ignorant of the contrivance of Sig. Romas, who interweaves the string which holds his kite with thin metallic wires, the same thought occurred to me the more naturally, as I was then exploring the accidents of the weaker electricity that takes place in serene weather.

1079. But it soon happened with me, as is commonly the case, that one observation created a wish for others. Having ascertained the electricity of windy clear weather, I thought of employing sky rockets to explore its state during a calm; and at last, in order to make constant and durable observations on its variations, I had recourse to exploring wires, as I related in the above quoted letters to Sig. Beccari.

1080. Besides the above expedients, I had moreover tried, though without success, a pole with an electrometer; however, I find that Mr. Ronayne has employed it very successfully for investigating the electrical state of fogs; which electricity I had at first explored with sky-rockets, and I have afterwards ascertained its nature by means of my extensive exploring wires.

1081. Till the year 1754 I had frequently written to Sig. Beccari concerning aerial *artificial* electricity, and its laws; the contents of my letters I published in the year 1758. I thought
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in the beginning, that the electricity which was propagated in the air of my room, adhered to the substance itself of that air; and I judged in the same manner of that which takes place during serene weather. As I had not then sufficiently considered the weakness of such electricity, I thought it could be observed, like the other kind of atmospheric electricity, with an electrometer not insulated. To that end, I raised a long pole on the top of a house, on the extremity of which a stick was placed, with two thick linen threads hanging from it. However, I reaped no information from the experiment, partly because the threads entangled together, partly because I did not visit them frequently enough, and did so only in very clear weather, when they were least likely to indicate any thing.

1082. But, to say the truth, the aerial and *artificial* electricity I raised in the room, was in reality an *aereo-vaporous* electricity (890); that is to say, it was diffused through the moist effluvia which swam in the air in it; and in like manner, the natural electricity of the atmosphere during clear weather, resides chiefly in the vapours diffused in it; and as such electricity, when the weather is completely clear, is but weak, it cannot, like the electricity I raised in the air in the room, be observed with electrometers that are not insulated; for these two electricities are respectively *proportioned to the deficiency of fire which the excess in the vapours may, in both cases, raise and maintain in the air in which the electrometers are immersed*. Now the natural excess of the atmosphere, when the weather is completely clear, and cold, requires more than an whole minute to be able to reproduce any slight electricity, by means of a lofty exploring wire. Electrometers, therefore, that are not insulated, cannot indicate the electricity of clear weather, or at least cannot indicate it with that constancy with which long and lofty exploring wires, that are exactly insulated, are able to do, which gather in themselves, unite and retain such electricity; so that it at last rises to a sufficient degree of intensity to be discernible. And now, that to these observations, I have joined those made with a pole, like that

contrived by Mr. Ronayne, I find my reasoning to have been conformable to facts.

1083. But it is time to conclude this discussion of the methods I have employed; now I proceed to relate what new discoveries I have been able to make, especially concerning the daily period of atmospheric electricity during serene weather.

1084. Proposition X. *In the morning, if the hygrometer continues to indicate a great degree of dryness, which is equal to, or little less than, that of the preceding day, then, even before the rising of the sun, an electricity takes place, which is manifested by junctions, adhesions, or even a divergence of the balls, and is proportioned to the absolute degree of the dryness in the air, and the smallness of its difference from that of the preceding day: if no such great dryness obtains, no discernible electricity takes place before, or even for a little while after, the rising of the sun.*

1085. As the weather during the night is commonly damp, so it is a pretty common case, that, before and after the rising of the sun, no electricity obtains. The following are the days in which I have found that an electricity took place before sun-rise, during the space of three months, the time from which I have resumed my observations, that is, from the 11th of April, to the 11th of July.

April 11, 12, 13, 14, 18, 20, 21, 26, 27, 28, 30.

May 9, 17, 24.

June 1, 4, 24.

July 3.

1086. On the 14th of April, the long drought still obtained, which had prevailed during the preceding months, though it then began somewhat to abate; therefore, on the first four mornings when I renewed my observations, I found an electricity obtained before the rising of the sun, and I think I would have found it equally constant during the preceding months, had I observed. In May and June, some dampness in the air arose at night, and the same continues to be the case in July: this moisture is brought over by easterly winds, especially the South-east wind; this latter

latter has several times caused my hygrometer to fall 10, or 12 degrees, and then rain has followed.

1087. From the whole of my numerous observations, I find, that it is rarer to meet with an electricity before sun-rise in summer, than in winter; especially, if the dampness from hoar-frost is prevented, as I have three times experienced during the month of January, 1769. I had raised a long pole on the roof of the Observatory, and on it a glass-stick, a foot and an half long was fixed, which was protected by a deep and narrow umbrella of tin, so that the hoar-frost could not so speedily coat it. The iron wire which proceeded from the above glass stick, was kept stretched by a silk string, fastened to a chimney, the remaining heat of which somewhat preserved its insulation. Lastly, this wire was bent, and was continued so far as to the inside of the observatory, through a window situated on the South side of it. Having three times during that month, tried, before sun-rise, the electricity of this wire, the extremity of which I held with a silk string, which was kept dry by some fire, I found a pretty considerable one obtained, which one time out of the three, afforded pretty frequent sparks, though for two or three minutes only, that is, as I think, when the hoar-frost was beginning.

1088. With respect to the observations I have made these three months, the result has been, I. That I never found an electricity obtain in the morning, unless the hygrometer indicated a degree of dryness beyond -12° . II. Commonly, when I have met with an electricity before sun-rise, the hygrometer was not fallen of an whole degree under that exhibited on the preceding evening. III. Once, that is on the 14th of April, I found the hygrometer fallen three degrees, but then it was with great difficulty I could discern a few electric adhesions; to this add, that, when they were taking place, a gentle North-west wind began to dissipate the dampness of the air, as I ascertained by means of my hygrometer with shreds of straw.

1089. However, in order properly to examine the connection between the dampness, or the dryness of the atmosphere, and the

the failure, or the prevalence of the morning electricity, one must take care that no strange dampness arises in the place of the observation. Thus, for fear my own perspiration during the night should disturb the electricity, at night I take care to transport the extremity of the deferent wire to the window of the next room to that in which I sleep, and to which I may, when there is occasion for it, speedily go and observe.

1090. Proposition XI. *In the morning, according as the sun rises higher, the electricity, whether it began before sun-rise, or only after, gradually increases. II. This gradual increase of the morning electricity begins sooner, according as the hygrometer continues, after sun-rise, to indicate a higher degree of dryness, and as such dryness more speedily increases. III. These increased both intensity and frequency of the electricity last, in serene days, in which no impetuous wind takes place, so long as the sun does not draw near the place of its setting, and the hygrometer keeps near the highest degree which it had reached. IV. When the sun is near its setting, and in proportion as the hygrometer begins to retreat, the intensity of the daily electricity lessens, and its frequency increases.*

1091. The truth of the above proposition will appear evident to every attentive observer, and I think I may dispense with producing any instances to support it. I will only remark that whoever will wish to be properly informed of the gradual advances, and diminutions, of the dryness of the atmosphere, must make use of pretty quick hygrometers; to that end I again recommend those with twisted shreds of straw: now I proceed to point out the different causes which produce variations in the daily period of atmospheric electricity.

1092. Prop. XII. *Though the hygrometer may indicate equal degrees of dryness, on the middle of the day, in different days, yet the frequency of the daily electricity may be greater in some of those days than in others, and is in great measure proportioned to the increase of heat which takes place in them: the electricity moreover arises, on such days, later in the morning, and fails sooner in the evening.*

1093. On the 11, 12, and 13th of April, the hygrometer stood very near -20° ; on the 28th of June, and 2d of July, at 2. 30' in the afternoon, it again rose to -20° . Now, though in the middle of each of those different days, the hygrometer indicated equal degrees of dryness, the operation of the electricity in some of them, proved extremely different from what it did in the others.

1094. In the three former days, before the rising of the Sun, an electricity took place which was manifested by motions, or adhesions, of the balls to my finger; and two hours after sun-rise, they diverged 4° , 5° , 6° . II. Nor did the electricity rise higher during the remainder of those days. III. At sun-set the degree of the electricity was scarcely lowered. IV. The slowness of it was moreover such, that, in the evening, after the sheet of brass between the balls had been touched, 30" at least, elapsed before a fresh adhesion took place; and in the middle of the day, an whole minute was requisite.

1095. In the two latter days, I. Bare adhesions scarcely began at 8 o'clock. II. At night, they ended before 7. III. But during each of these two periods, the electricity was renewed in less than a second. IV. However, in the middle of the day, the electricity rose to 8° , but still its frequency continued to be pretty considerable, and after 2" it began again to be manifested.

1096. And indeed, though in the middle of every one of these different days, the hygrometer indicated very nearly the same degree, of dampness, and the surface of bodies was equally deprived of moisture, yet the air was impregnated with very different quantities of it. A proof of this is, in the first place, supplied by the hygrometer itself. On the 11, 12, and 13th of April, the hygrometer did not fall, even during the night, under -19° . On the contrary, its variations during the two above mentioned warm days, were as follows,

28th of June, 5. 30' in the morning — 9. 5.
2. 30' in the afternoon. — 19. 3.

at

at 5. 30' on the following morning, — 10.

4th of July, 5. 30' in the morning — 12. 4.

2. 30' in the afternoon — 19. $\frac{3}{4}$.

at 5. 30' on the following morning, — 8.

1097. In these two last days the hygrometer therefore indicated a much greater dampness during the night time, than it did in the former. II. The air, of course, in the middle of those days, kept a greater quantity of moisture in a state of exact dissolution. III. And the heat of such days was undoubtedly the cause of this: in fact, the thermometer of Reamur rose to 24° ; whereas in the former, it did not rise above 10° .

1098. Hence, though the hygrometer exhibited the same degrees of dampness, in the middle of the former days, as in that of the latter, yet the state of the atmosphere was very different during the nights. Nay, it was so likewise in day time; and to such differences the operations of the electricity proved very conformable.

1099. And to begin with that part of the above phenomena which is most obvious, it is plain that the abundant moisture which, during the latter hot days, was kept in a state of exact dissolution, began, towards the evening, and as the heat gradually lessened, to descend and condensate itself near the surface of the earth; by which it weakened the insulation of the inferior part of the atmosphere, as well as that of the exploring wire: hence, the whole electric fire that remained in the atmosphere, every where diffused itself into the earth. As for the night time, no electricity was *manifested in it*, because the general deperdition of it still continued, and even increased, as the remarkable alterations of the hygrometer witnessed.

1100. However evident the above explanation may in itself be, I shall add a few more facts to support it. On the 23d of June, the hygrometer, from — 5 rose to — 17, 3; which denoted great dryness; for the 22d had been exceedingly wet, and I have constantly observed that one day is not sufficient to carry the hygrometer quite up to the actual degree of dryness, if the

the dampness was very great before. Conformably to the above great rise of the hygrometer, the electricity which, at 7 in the morning, was at zero, at 8, 30. had risen to 6° . and during the whole day, kept at 80° or very near. On the evening at 6, the electricity had fallen to 5° ; between 8 and 9, it fell to 2° ; at 10 o'clock it rose again to 5° , but it soon lowered again to 3° , to 2° , and there it remained till I ceased to observe, that is, till 11, 25'.

1101. On the morning I began again to observe at 3, 15', when I found that the electricity subsisted, and was manifested by brisk motions of the balls; and at sun-rise it increased.

1102. Now, this instance of the electricity continuing during the whole night in clear weather, is no very common instance in summer. It even happened then, that in that very night of the year, which is so celebrated with the vulgar, on account of St. *John's dew*, the atmosphere deposited scarcely any moisture. The thermometer, which, at noon, was near 23, at 11, 25', continued to stand so high as 19, 5; and at 3, 15' in the morning, it still stood at 19.

1103. The dryness of the above night was moreover rendered very conspicuous by the hygrometer: during the whole night it did not lower one degree under — 17, 3, which was the highest point it had risen to on the day before; and on the morning, before sun-rise, I found it at 16, 5.

1104. Again, on the 30th of June, the electricity continued pretty high during the night; at 10 o'clock, it stood at 2° , and correspondently to this, the hygrometer, from the highest degree of dryness, — 17, 4, which it had expressed on the day before, was scarcely lowered 0, 2: on the morning, at 4, 30', the hygrometer was lowered to — 15, and I accordingly found the electricity to have ceased; nor was it before 6, 10' that it began again to be manifested.

1105. Indeed it is no very rare thing, in summer, to meet with instances of the electricity failing about the time the sun sets, owing to a certain sufficient degree of dampness which then arises; and it is afterwards renewed, as the air, a few hours afterwards, again acquires a certain degree of dryness: but this kind

of electricity I here mention, belongs to that which I call electricity *of dew*, of which I propose to treat more explicitly, when I shall speak of the last part of the daily electrical period. However, the two above rare instances I have just now described, in which the hygrometer continued, during the night, to show a degree of dryness very nearly equal to that in the day, seem to me pretty plainly to indicate the cause why the daily electricity begins (as it indeed commonly does) later in the morning, and rises sooner at night.

1106. But to return to the greater frequency of the electricity in warm days, its difference from that which takes place on other days, is so great, that, on the very first year I began to observe, I perceived it: and of this fact, not only particular experiments, but the whole series of any observations that may be made, will furnish assured proofs; especially if such observations are made in calm weather, for, as we will soon mention, some particular winds increase the frequency of the electricity, as well as heat. Sometimes in fine summer days, I have suspended a pendulum for seconds, near the electrometer, which I touched with the same hand that kept the pendulum raised; my other hand was in the meanwhile placed near the balls of the electrometer, ready to make them manifest their electricity as soon as it should begin to take place; now, as soon as my other hand was removed, and the pendulum began to descend, the balls performed a vibration between my finger and the electrometer. On the contrary, in fine winter days, I have time to cross the room once or twice, before I find a new adhesion takes place.

1107. These great differences in the frequency of atmospheric electricity, I look upon as undoubted proofs of what has been advanced in Prop. VI. of the preceding letter; which is, that to that quantity of moisture which is brought by the air close to the exploring wire, the electricity of clear weather is proportioned, except in as much as this moisture lessens the exactness of the insulation. In fact, in warm days the air takes in and dissolves a much greater quantity of moisture, as we find from those great variations that immediately take place in hygrometers, when to
hot

hot days succeed very cool evenings; and this heat (as we may perceive with our naked eyes, and as I have still better ascertained with the microscope of the Count of Pertengo) keeps the above moisture in a state of continual ebullition. On the contrary, when the weather is both very clear and cold, to the slowness of the electricity corresponds a scarcity of atmospheric moisture.

1108. Nor is it only from such considerable differences of the electricity, as correspond to considerable differences of heat, that I am confirmed in the above opinion; but I am also from the farther circumstance of smaller differences in this electricity, also corresponding to smaller differences in the heat. In fact, on the morning of summer days, the electricity when it first begins, proves somewhat less frequent than a few hours after; which is no doubt owing to the less quantity of moisture which the air keeps at that time in a state of exact dissolution.

1109. With such hypothesis, the manner is also very contemporaneous in which the electricity both lessens and then fails, towards the end of the day, and as the night advances. In fact, even in cold and very dry weather, when the electricity is very weak, yet, as its intensity gradually lessens towards the close of the day, its frequency increases (or rather, its slowness diminishes) proportionally to the above decrease of its intensity; and this frequency keeps increasing, according as the dampness of the night more quickly augments, that is to say, according as the moisture which, during the day, was any how disseminated in the air, more speedily gathers near both the exploring wire, and the surface of the earth; the consequence of which is, that the electricity of the wire is more quickly annihilated, and also, that the same moisture then more speedily brings a small new electricity to it, which is proportioned to its imperfect insulation.

1110. If to all the above observations, we add the consideration of the peculiar electricity of *dew*, with which I propose to conclude, and that of winds, which, in proportion to their dampness, as I shall presently relate, increase the frequency of

the electricity of clear weather, we shall certainly find it as evident a truth as any in Natural Philosophy, that the moisture in the air is the conductor of this electricity.

1111. The electricity, however, either in summer and winter, does not always exactly keep pace with the quantity of moisture in the air, either in the night or the day. When I make my experiments during summer, in a shady place, they entirely fail, or only answer very imperfectly; though in order to make them succeed, I only need expose the whole apparatus to the sun. That is to say, the moisture in that air which is exposed to the sun, is more divided, is brought to greater rarity, and greater intervals take place between its parts than in that in a shady place; it therefore less weakens the insulation. Now, the exploring wire is in the day-time exposed to the sun, and in summer to a sun which is the warmer.

1112. Prop. XIII. *The friction of winds against the surface of the earth, is not the cause of the atmospheric electricity. II. Impetuous winds use to lessen the intensity of the electricity of clear weather. III. If they be damp, they lessen its intensity in proportion to the diminution they cause in the exactness of the insulation, both of the wire, and of the atmosphere.*

1113. Ever since the atmospheric electricity was discovered, I have endeavoured, after numbers of different manners, both in very dry, and in damp weather, to raise some electricity in deferent bodies, by means of currents of air artificially produced. Several times I have tried rapidly to turn four bands of gilt pasteboard, fixed like wings, to an axis insulated by means of sticks of glass. At other times I have for a while kept driving air against sheets of metal, with a pair of bellows, and sometimes, against bands of linen cloth, either very dry or wet, which were stretched within a frame, and insulated with silk strings. I have also tried to set fire to squibs which turned with great velocity around a stick, or peg, exactly insulated: now, neither in this peg, nor in the above mentioned axis, which consisted of an iron rod, nor in the above sheets of metal, nor in the bands of linen cloth,

cloth, could I ever perceive the least degree of electricity, both when the current of air took place, or after.

1114. Again, in the Letter addressed to his Excellence, Count of Scarnafigi, which is joined to those directed to Sig. Beccari, I demonstrated by conclusions drawn from accurate experiments, that the light which I had discovered to take place when a column of air precipitated itself into a vacuum, indeed was an electric light, but then that it never took place on the surface of deferent bodies, and only on that of thin insulating ones.

1115. Experiments of this kind are certainly very apt to raise doubts about the opinion which certain *closet* philosophers so gratuitously propagate, which is that the whole atmospheric electricity is produced by the friction of the air against the surface of the earth.

1116. Such opinion is besides abundantly confuted by the observation which forms the second part of the present proposition, which is, that winds commonly weaken the atmospheric electricity during clear weather: with respect to this general *assertion*, I shall here enter into some particulars.

1117. In three different cases, I have found the electricity of clear weather, intirely annihilated by the wind. In the first place, it has happened, when owing to the impetuosity of the wind, the kite with which I explored the state of the electricity, hath in consequence of its tail not being sufficiently loaded, taken a plunge, and been brought near the earth. I describe one of those cases in my letters to Sig. Beccari (p. 106) in which the wind, besides being impetuous, moreover appeared to be extremely dry; but the same has also happened when the wind was evidently damp. I shall observe, by the by, that if the electricity in any degree arose from the friction of winds against the ground, its intensity would be found greatest near the surface of the earth.

1118. The above rare experiments made with kites, are moreover exactly conformable to the constant and universal observation, that exploring wires become impregnated with a greater

greater electricity, according as they are raised higher above the surface of the earth.

1119. But lastly, Sig. Count di Pertengo has obtained an instrument for me, from the very kind Sig. Marquis di Prié, with which I almost every day experience the inefficacy of winds to raise electricity. The instrument I mean is a fine umbrella made of silk cloth, with a handle of charred wood, which is coated over with sealing-wax: the Merchants in Paris who sell these kinds of umbrellas, call them *Paratonnerres* *. Now, though I have often presented this umbrella obliquely to a strong wind, I never happened to find the least electricity or motion in silk hairs, wetted with salt water, which I had annexed both to its middle parts, and to its edge.

1120. In the second place, I have likewise found impetuous winds to have destroyed the electricity of exploring wires, though the latter were placed very high above ground, and of kites, though I then made them rise to a great height: such impetuous winds arose at the time showery clouds were in sight, which they almost instantaneously dissipated. I find nine such instances, in the journal of my observations in the Valentin.

1121. Lastly, I have found impetuous winds intirely to destroy the electricity, when the weather was very clear, and quite dry. With respect to this, however, I shall inform the reader, that I have not judged of the dryness of such winds according to the method of the vulgar, who mistake wind for dryness. Indeed, wind carries off the moisture of such bodies as are more abundantly impregnated with it than itself is: hence winds generally dry wet clothes to some degree, and constantly promote the evaporation from the surface of waters: but winds of really great dryness, which draw off all moisture from the surface of bodies, are, at least in this country, extremely rare. Hygrometers of twisted straw often have indicated to me that certain winds

* An umbrella, or shelter against thunder; the above word is imitated from the French words *parasol*, or *parapluie*, umbrellas for the sun, or for rain.

contained moisture, which, from every other circumstance, I judged to be completely dry.

1122. In my experiment of the 29th of November 1756, which I have related in my Letters to Sig. Beccari (p. 129), that wind, which, almost in every respect, appeared to me completely dry, yet, very likely was not really so, unless perhaps in a few particular waves of it, and indeed I could now and then discern symptoms of moisture from it, though, as I say, they were not constant, and proved but very weak.

1123. The winds that prevailed on the high mountain of St. Michael, where I made experiments in March, the following year, were of still greater dryness. There, when the frequent impetuous West wind blew, the electricity grew exceedingly weak, or even was completely annihilated; and the three only times I saw it, under such circumstances, to be somewhat intense, was when its nature was altered (1008). And then, in the calm intervals that at times took place, the usual mild electricity of clear weather again insensibly arose. In the mornings, a very strong and continued East wind commonly rose, which presently destroyed the electricity of the wires. Four times, while this wind blew, I succeeded to make a kite rise to a pretty great height, by fastening a stone to the extremity of its tail, but it did not send me the least electricity. Such winds, I only said, were drier than that before mentioned; for as to being perfectly dry, I do not know whether any wind ever is so.

1124. Nor is it necessary to climb up to the tops of high mountains, to meet with such winds as are sufficiently dry, either to annihilate any perceivable electricity, or create in it such a peculiar decrease both of its intensity and frequency, as may demonstrate the truth of the above observation.

1125. Even in Turin, in the spring of the year 1757, on two occasions in which a strong West wind blew, I could not, owing to its dryness, perceive any sign of electricity in the flaxen threads annexed to the string by which a kite I had raised was held, except a few rare and weak motions.

1126. As to this place, Garzegna, where I use to experiment in Autumn chiefly, I never meet with any instance of wind that I might call perfectly dry, except perhaps once; the kite then happened to be raised pretty high for the short space of one minute; and indeed I could not then perceive any electricity. But in this present summer, three times a dry wind has annihilated, as I found, all electricity during the intervals of its greater violence, and that happened too, in the afternoon, when the electricity uses both to continue at such a degree of intensity as to keep up a divergence in the electrometer, and to be very frequent, owing to the heat in the air.

1127. Here follows the manner in which the last of the three instances I mention were set down in my Journal.

" July.	hour,	barom.	therm.	hygrom.	bandrole.
11.	4, 15.	26, 9. 5.	25.	- 20. 7.	E. N. E.

Wind, impetuous and dry, and of course warm. . . . The electricity has for these few minutes proved quite unperceivable in the electrometer; at 4, 36', adhesions begin again to take place, and I correspondently begin to find that the wind grows less dry, for it grows somewhat cool. . . . The hygrometer of twisted straw, which is situated in a place towards East, in ten or twelve seconds, retreats one degree from the point of dryness it indicated; and brisk motions of the balls already begin to take place. As to the hygrometer with a string, though I am always contriving corrections in it, when, the weather continuing the same as to heat and clearness, I find it to go beyond—20, yet, it reaches to-day to —20, 5.

1128. I have transcribed the above extract because (besides the mention that is made in it of the imperfection of the hygrometer with a string) it serves me as a natural transition to speak of moist winds. Even those which seem to be very dry, at last often introduce some moisture. Easterly winds, during these three months, which, when fair weather is settled, use here in Garzegna, to rise pretty strong about noon, at three or four in the afternoon, by wheeling to South-east, become damp to a
very

very perceivable degree; and in the beginning, that is in April, or the first days of May, though the electricity still continued to be of a pretty slow kind, they often remarkably increased its frequency, so that the balls of the electrometer, which, before this damper wind rose, acquired a fresh electricity only after 20", or 30" of time, now recovered it after every second.

1129. At other times these damp winds I mention have, at their first setting on, carried the electricity so far as 8° , or 10° , and at the same time they increased its frequency; but this has happened but seldom, and continued only for a short time. In Superga, an evidently very damp wind has, several times, made the bells ring for a considerable time; which is certainly in great measure to be attributed to the very great length of the exploring wire I used there, and perhaps also to the considerable height of that mountain.

1130. Here in Garzegna, where easterly winds must, as they come on, rub against a series of hills which are somewhat a-head of it, they, of course, diffuse into them great part of the electricity which they may contain: hence these winds lessen and at last annihilate the electricity, in proportion to the time they last, and to the moisture they bring along with them.

1131. To the above reasoning the observations I have constantly made in the Valentin, are very conformable: strong and damp winds, in that low place, soon dissipate the electricity.

1132. Thus have I laid down, most worthy President, the theory of the alterations which heat, and especially winds, create in the daily period of the atmospheric electricity. Here I might add, as in their proper place, a few considerations on the sudden electricity of *evening dew*, which in certain seasons, and on favourable days, grows complicated with the former, if I did not perceive that the many particulars I have entered into, in this letter, have already rendered it but too long: this length however, I hope you will excuse, partly on account of the subject, partly on account of the desire I entertained, while writing it, that it might convey a sufficiently clear idea of the subject, both to you,

O o o

and

and to the ingenious Members of this Society, so that they might judge how far the above observations are well grounded, as well as rectify them if necessary; for though I do not think myself intirely destitute of any degree of pride, yet, truth is the object I seek after.

*Garzegna di Mondovi,
July 14, 1775.*

LETTER III.

On the Electricity produced by evening Dew.

To the Same.

1133. FROM the beginning I thought that bare reasoning and analogy could demonstrate the existence of the electricity that is produced by dew. The first experiment that made me suspect that observation might moreover render it sensible to us, was that which I made with a kite, here in Garzegna, on the 18th of October 1756; in which, a quarter of an hour after sun-set, the string which held the kite supplied such a quantity of electricity that it could form both a *brush* and a *little star*, on two pins, respectively, which were properly directed to produce the above effects. (Let. p. 126).

1134. The above opinion is moreover confirmed to me by what I find written among the observations made at the Valentin. "Last night, the Gardner assures us, he has drawn ten successive small sparks."—I could not be on that spot during the nights.

1135.

1135. In March of the same year, on the mountain of St. Michael, I frequently continued up late at night, hunting after the electricity of evening dew, which I thought must obtain there, but I never could discover any signs of it: I imputed it, and pretty justly as I now find, to the strong continued winds that prevailed.

1136. In the Autumn of the same year, while Sig. Canonica continued the observations in the Valentin with his usual exactness, I went to Genoa, in order to make experiments concerning sea light, and take information on that subject, from seamen. It was only in the following year (1758), that I settled an exploring wire, 1000 feet long, here in Garzegna, with which I several times discovered, especially towards the latter end of October, that an electricity *of dew* took place, which was of pretty considerable intensity. Ever since that year I have observed and set down, that such electricity took place in clear and dry weather, during which no strong wind prevailed.

1137. In subsequent years I continued to observe the same electricity of dew, in those evenings in which the above united circumstances happened to take place. At that same time I reflected, and afterwards saw confirmed by observation, that that kind of electricity which prevails in the evening about the same time as the electricity of dew commonly does, and which, though it attains a less degree of intensity, yet manifests a pretty considerable frequency, is of the same kind with this latter.

1138. At Superga, during those evenings I could spend there, I only happened to observe this second kind of electricity. Sig. Prior Ceca could not so intirely dispose of his time, during those hours, as constantly to watch the motions of the electricity of dew, and he only paid attention to it when it became manifested to a great degree, and hastened from his room to observe it, only when he was called up by the sound of the electric bells; however, as I will relate in its proper place, he has several times happened to observe the real electricity *of evening dew*, though he considered it in a different light.

1139. Lastly, this present year, on the beginning of April, I have again repaired to Garzegna, in order to make new observations, (for it is by constant observing alone, that I learn how to observe properly) and it seems to me that, with respect to the electricity of evening dew, the few following propositions may be laid down as founded upon facts.

1140. Prop. XIV. *In cold seasons, if the sky is clear, no strong wind prevails, and a pretty great degree of dryness continues to obtain, an electricity of considerable intensity arises after sun-set as soon as the dew begins; the frequency of such electricity is moreover greater than that of the daily electricity, and it vanishes with great slowness.*

1141. Prop. XV. *In temperate or warm season, if the same circumstances as above take place, an electricity, entirely similar to the former, arises as soon as the sun has set; only, its intensity is not so constant, it begins with more quickness, it rises to a greater frequency, and ends sooner.*

1142. Prop. XVI. *If, under the above circumstances, respectively, the general dryness of the air happens to be less, then the electricity that arises in the evening, when the dew begins, is less, in proportion to the diminutions of the exactness of the insulation of both the exploring wire and the atmosphere that then take place; but then, correspondently to the greater quantity of dew, the frequency of the electricity is greater.*

1143. Prop. XVII. *The electricity of dew depends, it seems, on the quantity of the dew, as the electricity of rain depends on the quantity of rain; and the peculiar manner after which this dew takes place, influences the electricity, in the same way as does the peculiar manner in which rain likewise takes place.*

1144. Prop. VIII. *As rain, showers, Auroræ boreales, zodiacal light, have a tendency to begin afresh for several successive days with the same characteristic accidents, so the electricity of dew seems to have, as it were, an inclination to appear for several evenings successively, with the same characters.*

1145. Prop. XIX. *Let the air in a well closed room be electrified, that is to say, the moisture and other vapours diffused in it (887).*

II. Let a bottle filled with water colder than the air in the room, and insulated on a tube of glass, be raised pretty high in this room. III. Let care be had to preserve the insulation of the glass with warm clothes. IV. The electric signs that will arise in two threads suspended to such bottle, will exactly represent the electricity of dew. V. And they will exhibit the different manners after which this electricity takes place, according as the electrified vapours in the room will be more or less rare, according as the difference between the heat of the air in the room, and that of the water in the bottle, will be less or greater, and according as the insulation of the bottle will be more or less exact.

1146. The above propositions I have thus set down jointly, that the one may serve to add a new degree of clearness to the other. And, in order to throw still greater light on the subject, I have subjoined a compendious table of twelve different observations, the only ones I have had an opportunity of making concerning the electricity of evening dew, during the 120 days I have resided here since the 11th of April.

I. Months.	II. Days.	III. Electricity of dew.	IV. Its beginning, hour, min.	V. Hygrometer with string.	VI. Its variations.	VII. Variations of that with twisted straw.
April	11	18°.	7, 25.	19. 8	0 0	0 4
	12	16°.	7, 30.	19. 6	0 0	0 3
	13	15°.	8, 35.	19. 2	0 0	0 3
	17	12°.	7, 30.	18. 4	0 0	0 5
	20	18°.	9, 0.	18. 3	0 1	0 7
	26	15°.	7, 30.	17. 7	0 1	0 7
	27	12°.	8, 35.	14. 0	0 0	0 5
	28	12°.	8, 20.	17. 0	0 0	0 9
	9	22°.	8, 40.	17. 3	0 1	1 8
	19	13°.	8, 40.	18. 4	0 0	2 0
	22	23°.	7, 50.	17. 0	0 2	2 6
	23	20°.	8, 10.	15. 0	0 2	2 0

1147. The small quantity of observations in the above table, instead of proving the existence of the electricity of dew, seems rather

rather to give rise to objections against it. In fact, since the fall of dew is so common an accident, how has it happened, that during 120 days, the electricity of dew has taken place only twelve times?

1148. But in the first place it is to be observed, that an electricity of this kind, somewhat intense, is only to be observed when the general dryness of the air is very great, as is evident from the column IV of the table above, in which the state of the hygrometer with a string, at the time of the rise of this electricity is set down,—and also when such great dryness is of a permanent kind, as is likewise manifest from the steadiness of the above hygrometer during the time the electricity continued to be particularly strong (its variation having been either very small, or zero) and from the state of the hygrometer with twisted straw during that same time, the variation of which was but small, and has likewise been set down in the VIth column. Now, such evenings as are altogether clear, greatly and permanently dry, and free from any strong wind, are upon the whole but very rare.

1149. The evenings, during the whole warm season, ought therefore to be excluded from the account, as wanting that great, and moreover permanent, dryness above mentioned. In fact, the copious moisture which the air dissolves during the day, lessens the insulation so much, that the frequency of the electricity greatly increases, though not its intensity; and this moisture, when first condensed by the coolness of the evening, scarcely allows any sufficient insulation to subsist for the electricity of dew to become manifested. The twelve times therefore, in which I have, since the beginning of this season, had an opportunity of observing an electricity peculiarly intense, ought not to be reckoned out of 120, but only out of perhaps less than fifty days. If instead of coming here to Garzegna, in order to observe, on the 11th of April, that is, on the last days of the long drought that had prevailed, I had come on the beginning of March, I do not doubt but I might be able now to reckon a much greater number of observations, in the same manner as I have formerly been, in more proper seasons than this, though I spent a much less number of days in observing.

1150. Besides, it ought to be observed that I have set down only those days in the above table, in which the electricity has been sufficiently strong to carry the divergence of the balls to twelve degrees, or above, as is marked in the 2d column: I have purposely confined myself to such days, because the electricity I mention differs greatly from the common daily one, and is thereby very apt to engage the attention of an observer. Whenever an electricity arises in the evening, after the ceasing of the daily one, though it may prove much less than the above, and even be merely manifested by adhesions of the balls to my finger, yet it incontestably arises from the dew, since it is constantly proportioned to the quantity of this latter; and such electricity arises every day in which a sufficient dryness in the air, that is a sufficient insulation, obtains, and when no strong wind blows.

1151. This mention which I make of dryness in the air, and of insulation, leads me to give the solution of a difficulty concerning the electricity of dew, that seem to arise from the inspection of the columns V. and VI. That is to say, if so strong an electricity as above obtained, consequently so considerable a quantity of dew fell on the evenings of the twelve above mentioned days, how came it to pass that so small variations took place in the two hygrometers as are set down in those two columns? The hygrometer with a string remained several days without any motion at all; and the hygrometer with twisted straw, frequently moved only a few parts of a degree.

1152. In the first place, it is evident from the variations in the hygrometer with twisted straw having been greater than in that with a string, that the steadiness of the latter was then owing to its sluggishness merely; and this I have, besides found confirmed by a number of other facts. Neither are we to think that the hygrometer of twisted straw itself, was totally free from the above defect; besides that, as it was placed in a much lower situation than the exploring wire, it was affected later by the moisture of the dew.

1151. In order to judge of the quantity of moisture that is necessary for making the twisted straw advance half a degree, I

try

try to breathe upon it several times consecutively; when I observe, I. That at the first breathing it makes some little motion. II. But it moves so far as the whole half degree, only when I have breathed four or five times upon it. III. If, after the first or second breathing, I cease to breathe, the hygrometer constantly shows a tendency to move back. IV. When I breathe upon it, for the sixth or seventh time, then it, as it were, leaps at once to the distance of an whole degree.

1154. From these different accidents I conclude, that, even in the hygrometer of twisted straw, a kind of sluggishness obtains,—that some time is requisite to enable the moisture to penetrate into the inmost pores of it, and dilate them,—that the quantity of moisture necessary to make this hygrometer move half a degree is not quite inconsiderable,—and lastly, that the dampness must be somewhat continued, else the hygrometer will move back.

1255. But when I moreover consider that the electricity from the dew grows more manifest, every other circumstance being equal, in proportion as the length of the exploring wire is greater, and that the length of that which I use, surpasses eight hundred times the length of the above twisted straw, I understand how the lowest degree of dampness which the latter is able to indicate, is sufficient to create and maintain a pretty considerable electricity in the exploring wire.

1156. The above fact I more particularly discover when I observe the kind of proportion that obtains between the different manners in which electricities of strong kinds take place, and the then variations of the hygrometer with straw. For, as I have said in Prop. XVII. and as I observed in April last, when the season was above the temperate degree, the electricity of dew (except that it is always of the excessive kind) seems, in its different changes, to follow proportions much like those which take place between the electricity of calm mild rain, and that of rainy stormy weather.

1157. In fact, on the three first evenings in which I have observed in April, I found that as the twisted straw scarcely could move three or four tenth parts of a degree, so the electricity from the
dew

dew rose with great quickness, it lessened very slowly, and when annihilated, it did not rise again to the same degree, or nearly, but after fifteen minutes of time: at 11 o'clock 30', a divergence of 12 or 10 degrees continued to obtain; and on the morning, before the rising of the sun, pretty quick adhesions still took place, and a great dryness still continued to prevail, so that the hygrometer with a string had not moved half a degree.

1158. The very reverse happened in my last observations in May. I will produce as an instance that I made on the 22d. I found the electricity to have risen to 15, 16, 18, and 23 degrees; and between 7 o'clock 50', and 8 o'clock, that is, in ten minutes of time, it fell to 15: since that time it kept decreasing and increasing between 8 and 15 degrees; and by farther successive variations it was at last, about 11 o'clock, brought to only a small divergence. Now, all the above variations of the electricity corresponded to variations in that quantity of moisture in the air, which took place at that time, as was indicated by the twisted straw, which now and then started back towards dryness. With regard to the frequency of the electricity during that evening, I observed, at 10 o'clock, that two seconds after touching the electroscope, the balls began to diverge afresh.

1159. The *consistent* variety of the above accidents fully demonstrates the constant connection that obtains between the dew and the atmospheric electricity; for the manner after which dew takes place, varies as regularly as the manner of rain does, according to seasons. Nor is it necessary that the dew should be very copious in order to produce a strong electricity; on the contrary, a very great quantity of it would at last destroy the insulation of both the wire and the atmosphere. The electricity itself of rain, the intensity of which is so superior to the latter, likewise requires that the exploring wires should be in some measure insulated; and in proportion as the rain becomes more extensively diffused, this electricity grows less sensible; and it only recovers its former intensity, when a change takes place in the nature, or rather the manner of the rain. Now, dew is constantly diffused in great plenty towards the orbiting hemisphere,

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and

and its electricity may be compared to that of a very rare and subtle rain; and it therefore requires some degree of insulation should obtain in that part of the atmosphere which is little raised above ground, in which alone we can observe it: dew must therefore be in small quantity, that it may not diffuse away its own electric fire.

1160. The *daily* electricity, to sum the whole in few words, is therefore, like an electricity of a very rare fog, which rises, grows dilated, and thus lessens continually less and less the insulation: the nocturnal electricity is like an electricity of rare and subtle rain, which descends, gathers together, and continually lessens more and more the insulation. Hence the daily electricity is of a continuous kind; the nocturnal frequently fails; and it only attain its greatest degree of intensity, when the increase of that moisture which is the conductor of it, happens to take place without injuring the insulation.

Garzegna di Mondovi,
April 2, 1776.

P. S. The time at which the strong electricity of evening dew begins, varies very irregularly, as may be seen in Prop. III. Sometimes I have found it to begin before sun-set; but then, several clouds, pretty high and thick, lay in the part of the West, and the thermometer, as well as the hygrometer, anticipated their usual variations. At other times the same has only begun after eleven o'clock at night; nor have I always been able to find out any satisfactory reason for it.—I likewise think that the *morning* dew under proper circumstances, gives rise to a peculiar kind of electricity. The three observations on hoar-frost, which have been mentioned in a former place, may be considered as proofs of this; however, in order that this electricity may become manifest, care must be had that the preceding evening dew hath

hath not injured the insulation of the apparatus. In the above mentioned observation on the 23d of June, I think I discerned an electricity of morning dew: the dew of the preceding evening had been exceedingly rare, as I found by means of the hygrometer of twisted straw; on the morning at 3 o'clock, the balls of the electrometer scarcely manifested any tendency to adhere to my finger, and yet at four, they briskly vibrated between it, and the brass plate that stands close to them.

F I N I S.

ATMOSPHERIC ELECTRICITY, 60.

It is not unusual to find the intensity of the atmospheric electricity on the 1st of June, 1871, to be about 100 units. This is a very high value, and is due to the fact that the atmosphere is very dry at this time of the year. The intensity of the atmospheric electricity is also very high on the 1st of June, 1871, to be about 100 units. This is a very high value, and is due to the fact that the atmosphere is very dry at this time of the year.



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